THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES, AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. M.R.A.S. MUNICH, M.G.S. M.A.S. F.S.A. EDIN. AND PERTH, M.S.L.I.N. OF FRANCE, &c. &c. &c.

"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. LVII.

For JANUARY, FEBRUARY, MARCH, APRIL, MAY, and JUNE, 1821.

LONDON:

PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST, REES, ORME, and BROWN; HIGHLEY; SHERWOOD and CO.; HARDING; UNDERWOOD; SIMPKIN and MARSHALL, London: CONSTABLE and Co. Edinburgh, BRASH and REID; DUNCAN and PENMAN, Glasgow: and GILBERT and HODGES, Dublin.

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our observations relate to the whole of that space; which he would therefore divide into *four* equal parts, to each of which a set of observations should distinctly relate. I suppose, in order to obtain far better comparison, the heat of the day, the cold of the night, and the two intervening spaces of middle temperature, as also the duration of other phænomena, with reference to these four periods. He deprecates the objection of making the observations by this means too laborious, though it is clear, that only persons of fixed or sedentary habits could comply with such a rule; which *for that reason* it would not be advisable to make a *general* one.

With regard to the variations of the barometer and thermometer, he thinks that, for want of knowing the elevation of the place above the sea in the former case, and its annual mean temperature in the latter, we are often at a loss duly to appreciate the *absolute* measures, usually given of the height of each in the scale: he would therefore have the variations all put down *relatively*; that is, the barometer at such a time so many tenths *plus* or *minus*, with reference to a fixed *mean* height; and the temperature in like manner so many degrees *plus* or *minus*, with reference to a given *mean* temperature. This method would accord perfectly well with the mode of expressing the variation by *curves*, of which I gave a specimen, with a view to a particular object many years since, in the Philosophical Magazine *; and which I have since applied more extensively in the second volume of the Climate of London. But, before it can be brought into successful practice for any given station, we must have *for that station* a sufficient series of observations with good instruments to enable us to fix pretty accurately *its local mean*, as to both instruments; which being obtained, such *plus* and *minus* results would speak a very intelligible language, and in the case of *curves* being used with a given scale, *an universal one*.

The writer of the letter proceeds to observe, that the mean temperature of a year, taken as a whole, has a certain influence on its productiveness in an agricultural sense; but that the *irregular distribution* of the heat, even in a warm year, may render it a barren one. To pursue the inquiry into this interesting part of the subject, he views the day whether in winter or summer as warm or cold, with reference to the mean heat of the decade in which it stands; a method which may sometimes give more *warm days* in the winter than in the summer half year.

In order to the establishment of a common mode of graduation in meteorological instruments, he proposes that decimal divisions of the scale be adopted in all cases. As he employs

* Vol. vii. p. 365, &c.

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PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *On the discordant Opinions delivered by the Chemists who gave Evidence on the Trials of the Insurance Question of* SEVERN, KING, *and* Co. versus *the Fire Offices. By M.* RICARDO, *Esq.*

To Mr. Tilloch.

SIR,—FOR commenting upon the discordant opinions which were given on the trial of Messrs. Severn and Co. relative to some of the properties of oil, and which have been put on record in the last Number of the Philosophical Magazine, it is unnecessary for me to offer any apology to those gentlemen the correctness of whose doctrines I have presumed to call in question. Opinions upon scientific subjects, or indeed upon any other, which are publicly promulgated, become public property, and are open to the observations and criticisms of any one who chooses to descant upon them; and in freely canvassing such opinions, it is always to be understood that the doctrines only are criticised and not the persons advancing them; nothing personal is meant: that estimation in which, as men of science, they are so highly held, and that respect to which they have become so justly entitled, and which no one feels more warmly than the individual who is now addressing you, are not in the slightest degree lessened by the casual advancement of doctrines which on examination may not be found perfectly tenable. The question at issue, however, is not the truth or falsehood of an hypothesis; but it is as to the existence of facts, facts that no doubt may and will be clearly proved to the satisfaction, or I should rather say to the conviction, of all parties. Hypotheses or theories may be disputed, but the facts themselves cannot be disproved; and whether oil is decomposable below a certain heat, whether it gives out inflammable vapour, and whether it is easily or difficultly heated, may be as clearly proved, if the trial is made unbiased by party feeling, as any problem in mathematics.

The observations which I propose to make are upon the evidence which was given on the trial, without any reference to experiments that may have been subsequently made; and I shall

commence them with an examination of the second points, as stated in your last Magazine, which each party wished to establish; the gentlemen on the one side maintaining "that oil kept at a heat of 360 degrees for two months, underwent no change whatever, excepting becoming darker and thicker; that by such operation it did not become at all more inflammable." The gentlemen on the other side asserting, "that oil kept at the heat and during the time above mentioned did become changed, that partial decomposition took place, and that it became more inflammable." Here then there is a decided contradiction of facts. The above assertion on the plaintiffs' side was made by Dr. Thomson, and was corroborated by most of the other witnesses, with perhaps some slight modifications. They all allowed that an aqueous vapour was given out, which condensed and fell back in the boiler, and that the blackness of the oil was occasioned by carbon being deposited. This aqueous vapour, as it is termed, could not be the result of water mechanically combined with the oil, because the whole of that, if there were any, would be expelled before it arrived at the heat of 360 degrees. Now this vapour continues to be produced the whole of the time it is submitted to this heat. Aqueous vapour cannot be formed from oil without that oil undergoing some decomposition. The hydrogen and oxygen that leave the oil to form this vapour must disarrange its elementary constituents, and the residue must be a compound of a new order. Carbonic acid gas, it is stated, is given out, which cannot be produced without some further change taking place. Thus, Sir, on their own admission, we see that oil does undergo a change more than merely in colour and density, and that a decomposing process is going forward all the time it is submitted to that heat. It is somewhat surprising, that men to whose opinions so much importance is attached, and who are looked up to with so much deference, should not have made themselves acquainted with every circumstance that was necessary to enable them to give a correct opinion upon every point on which they might be examined, particularly when so much time was allowed them between the two trials to gain all the information possible. That which was termed aqueous vapour, is in fact not simply aqueous vapour, but combined with an inflammable vapour and a large portion of acetic acid, which is formed in the oil: their calling it aqueous vapour at least demonstrates that they assumed it was so without proving it; and if they admit assumption in one instance, their opponents may justly charge them with it in others; for *they* ascertained the vapour to be acid, and that it was acetic acid *.

All

* It may be said, this is not a new fact; as it was well known, and had been noticed in chemical works, that acetic acid comes over with the vapour

that

All these circumstances combined, prove that what the plaintiffs' witnesses asserted, that oil undergoes no change, is not correct; for aqueous vapour, combined with acetic acid and carbonic acid gas, is formed, and carbon is deposited. This could not take place, and I think no one will dispute it, if the oil did not undergo decomposition.

Of its being rendered more inflammable by the change it has undergone, I can only observe, that on this point the evidences of the plaintiffs' witnesses are at variance among themselves. Mr. Wilson, Dr. Thomson, Mr. Parkes, and others, observed, that there was no difference between old and new oil, as they were termed, in their combustibility. The latter said that old oil became less combustible; and this he did not state from actual experiment, but he assumed it because the oil became thicker. Mr. Brande on the contrary said, that he obtained inflammable vapour 50 degrees lower from old oil than from new; and Mr. Accum stated, that from oil that had been heated for some considerable time he had obtained inflammable vapour as low as 460 degrees; but he did not attribute its inflammability to its being old, but to its having been heated in a leaden vessel, and having dissolved a portion of the lead which, *in his opinion*, rendered it more combustible. Without adverting to any of the assertions made on the other side, as to the change which oil undergoes at the continued heat of 360, I think I have proved on their own admission, that oil does not remain the same; that a decomposition does take place; and that, new compounds being formed, it must necessarily undergo a greater change than merely becoming darker and thicker.

The next point for consideration is the change which takes place in the combustibility of oil at a higher heat. At 600 degrees, it is stated by Mr. Wilson on the plaintiffs' side, a small quantity of vapour is given out, but none below that heat: if a light were applied to it, it burned with a blue lambent flame and soon went out. About a foot high it would become condensed, and fall back again as oil. This was corroborated by Dr. Thomson, Mr. Brande, and many others, with slight modifications. The feeble testimony of so humble an individual as myself, in opposition to such high authority as that which I have already quoted, or in addition to the assertions of the equally high authority on the other side, may have but little weight with those who have already formed, or are to form, their judgement upon this subject: but it was a matter of no small surprise to me, whilst in Court, to hear that statement made, when but a very few days

that is formed during the process of heating oil. This does not alter my objections; but renders it the more extraordinary, that with the knowledge of this circumstance, it should be asserted that oil undergoes no change

before

before I had witnessed the violent and incontrovertible effects of oil ignited at 600 degrees; the vapour was lighted before it arrived at that heat, but I have said 600 degrees because it did not exceed that, the range of the thermometers terminating at that point, and which were not found broken after the experiment was finished. From a tube affixed to the boiler the flame ascended upwards of four feet in length, striking against the wooden roof of the place where the experiment was tried, and which for some considerable time baffled all the attempts to extinguish it, and made every one present seriously alarmed for the safety of the building: the fire was raked out, pails full of water were thrown into the fire-place; wet cloths were put on the mouth of the tube, to no purpose; the cover of some pot was at last placed upon it, which dispersing the flame laterally prevented its ascending so high; and in that state it continued burning for some considerable time. With this occurrence fresh in my mind, it was not to be wondered at that I should have listened with astonishment when I heard it asserted by Dr. Thomson, whose authority I had always looked up to with so much deference, that a fire twenty miles long placed under such a boiler as the one used by the plaintiffs could not make it dangerous.

This experiment, which I witnessed in company with those gentlemen who were subpœnaed on the part of the defendants, by the invitation of Mr. P. Taylor, at whose laboratory it was made, was one of many others tried by them, and all of which were attended by the same results. This it may be said is assertion against assertion; but the fact, whether the vapour of oil at 600 degrees gives out only a blue lambent flame easily put out, or a continuous one most ungovernable and with great difficulty extinguished, may be very easily proved.

The next point of difference is the length of time necessary to heat oil,—the one party stating that it would require eight or ten hours with great difficulty to bring it from a safe to a dangerous point; the other, that it may be effected without any difficulty at all in twenty minutes. I should observe here, that there is no detail of any experiment on the side of the plaintiffs, to prove the fact which they have stated; for the trifling attempt at heating oil over an Argand lamp, where the radiation of heat when the oil arrived at a certain temperature kept pace with that emitted from the lamp, is no proof at all; but I do not recollect any statement having been given of its being tried with a fire under the boiler with the express view of ascertaining that point: their endeavour was to keep the oil at a temperature below 360 degrees. But on the other hand the fact has been proved in six or seven public experiments, where in a boiler constructed upon the model of the one used by the plaintiffs, with a fire-

fire-place proportionably less, with a depth of oil nearly twice that which the large one contained, oil in twenty minutes, with a moderate fire, was heated from a safe to a highly dangerous point. Now, Sir, where two parties are equally entitled to credibility, we should certainly say that one positive assertion was worth a hundred negative, and that the detail of absolute facts must take place of mere conjecture.

What was said concerning a leakage in the boiler was hardly worthy the consideration of a chemist. A cook-maid could have answered that question equally well ; and if she were asked whether a hole in her frying-pan, and the consequent leakage of her dripping, would have any effect in raising or extinguishing the flame, she would tell you proverbially that "all the fat was in the fire," and leave you to draw your own conclusions.

The next subject for consideration is the production of Dippel's oil, which Dr. Thomson asserted could not be obtained from whale oil in a similar boiler to that used by the plaintiffs, and that passing it three times through a red hot tube would not produce it. Whether the latter mode would produce it, or not, I am unable to say; Dippel's oil is stated to be highly inflammable, very volatile, considerably lighter than common oil, and its boiling point is 180 degrees. A sample of oil was produced in court by Mr. P. Taylor which had all the properties above enumerated. It may be denied that this was Dippel's oil, because it is not produced from a solid animal *substance ;* but it clearly possesses all its properties, and it is needless to cavil about names. This was procured by the redistillation of a portion that had been distilled from the boiler in which the experiment was tried. The inference to be drawn from Dr. Thomson's statement was, that it required great difficulty to produce this highly inflammable oil. But it is clear, that if the vapour at 600 is condensed and falls back again, it must contain this inflammable matter disengaged in it, which may be driven over at a heat much below 360. The maximum of heat is here given ; but a set of experiments will be tried, to prove among other things at how low a heat this oil may be in the first and second instance produced.

The next point for consideration is the inflammability of sugar. The assertion which Dr. T. made, that sugar next to gunpowder was the most combustible substance in nature, must be considered, as well as the fire of twenty miles extent, as one of those strong declarations made without thought, and without considering the extent of their meaning, and which is intended to convey an opinion as strongly as it can be conveyed. Had the same opinion been given in a less forcible manner, coming from such a source, it would have been open to the same criticism. Hyperbole is not wanted to give force or authority to any thing that

Dr.

Dr. Thomson may say. Perhaps it may be difficult to disprove what has been said, that boiling of sugar is more dangerous than heating oil—that sugar boiling over would catch fire and burn as it ran along the floor—that mixing it with water rendered it more dangerous;—such assertions could only be met by counter assertions. But I would ask those gentlemen who made them, to reflect calmly, without letting their judgements be biassed by their prejudices, and say whether they really believe that syrup could, in the state in which it is boiled in the sugar-houses, and as the fire-places are constructed, on boiling over, possibly set the building on fire? whether it would burn as it ran along the floor? and whether mixing water with sugar adds to its combustibility? The evidence given by Mr. John Martineau on this point, which for clearness and perspicuity was not exceeded by any evidence given on that trial, might, I should have thought, have set that point at rest. He had watched the progress of sugar boiling upon a large scale with the eye of a man of science—he had traced it to its most dangerous point, and yet found it perfectly controulable; and though this gentleman might not boast of holding that rank as a general chemist which many others do, yet on this particular point I think he might be considered superior to them all. The experiments of Mr. Brande proved that at a certain heat the vapour issuing from sugar was inflammable; but long, very long before it arrived at that point, the sugar was spoiled, and rendered unfit for any purposes for which sugar refiners could use it. The neat and highly ingenious experiment of Mr. Children (whose name and character as a man of science entitled him to a more courteous observation than was made by the Solicitor-General, who styled him a mere parlour chemist) proving the inflammable point of sugar by phosphorus, is not at all at variance with Mr. Brande's trial; it demonstrated that long before sugar became dangerous, it became completely spoiled.

I have now gone through all the points on which the two parties have differed, and the differences must be determined by future experiments. Those who have advanced opinions with a conviction on their minds of their correctness, will not sit quietly under the imputation of having promulgated erroneous ones, particularly when they state these opinions to be facts founded upon the results of actual trials; and such facts cannot be disproved by mere hypotheses; they must be met by other experiments, not tried for the purpose of obtaining a particular end, but every end they are susceptible of. The dignity of chemical science has suffered much by this trial; but, though this is to be lamented, we may hope that the science will be greatly benefited, by a new field being opened for future investigation that may lead to results of great importance.

It

It may be said, that in the observations I have made, it has evidently been with a leaning to one side, and from my acknowledgement of having been present at one of their experiments with a strong bias to that side. If it is so, it is the result of conviction. With the best judgement I am capable of bringing to this investigation, the evidence on the one side appears to me infinitely more conclusive than on the other; and what I observed as an eye-witness, I cannot disprove to myself.

The assertion of the Solicitor-General, that the party opposed to him tried the experiments to prove how dangerous this mode of heating sugar was, is certainly not incorrect; but he did not properly state how they proved it, when he said that Mr. Farraday admitted that the fire was strongly urged at the last. I think he exceeded that gentleman's admission; if I understood Mr. F. rightly, the fire was not urged vehemently, and every thing done to heat the oil as quickly as possible; but that, having been obliged to use great care to keep the heat so low as not to allow the contents of the boiler to exceed 360, when they wanted to raise it they were necessarily obliged to increase the fire from the low point at which it had been kept: but as an eye-witness, I can say, not more than if they had wanted to heat water or any other fluid. On the other side the experiments were tried to ascertain how safely it might be used, and from taking precautions to keep it so, they did not attend to its danger; and trying only by halves, from the result of such knowledge and the imperfect information it conveyed, they were induced to advance opinions which, I have no doubt, at some future time, they will regret they ever uttered. It is impossible for any man, were his life of three times the usual extent, to go through all the experiments of which chemistry is capable; and therefore many opinions that were given were advanced not on their own, but on the authority of others. The properties of whale oil are still very imperfectly known; its highly offensive nature, and the inconvenience attending any experiments upon it, would deter many, who had not a strong motive, from making it a subject of choice. Mr. P. Taylor* had that motive, and I believe no one possesses more accurate information on this subject than he does, although even his knowledge is still very imperfect. The investigation of oil in the production of gas has long occupied his attention, and a further investigation in such hands must lead to important conclusions.

It is with great deference I have presumed to call in question opinions of men so justly and so highly estimated for their great

* I may be allowed to observe here, that the character and talent of this gentleman as a man of science are equal to any; his capability in planning experiments, and his ingenuity in forming just and conclusive deductions from their results, are perhaps superior to most.

talents; but the greatness of these talents renders it the more necessary, as it gives a currency and a sanction to errors that emanate from them; nor should the obscurity of an individual deter him from pointing out such errors when he thinks he has detected them.

I cannot conclude these observations without bearing my warmest testimony to the fairness, the candour, and perfect good humour, with which the experiment I witnessed was conducted. Mr. Dawes, the respectable solicitor, whom I had not the honour of knowing till I met him on this occasion, seemed only anxious for the truth, the strict impartial truth, and I am sure would have been as much above taking any quibbling advantage, had it been offered, as the gentlemen who acted with him were above offering him any. I have been induced to make these remarks, from many rumours that have been afloat of the motives which induced some gentlemen to give the evidence they did. It is to be most sincerely regretted that upon a question,—as far as they were concerned,—purely scientific, any personal insinuations should have been thrown out that must have wounded the feelings of those who knew they were as unjust as they were untrue. Let us hope that in the further pursuit of this subject, or indeed any other connected with science, a better feeling will prevail, and that the same desire will actuate all,—that of promoting its benefit to the utmost.

Bow, Jan. 11, 1821. M. Ricardo.

II. *On the Pyroligneous Acid, its Manufacture and Uses. By Dr.* Wilkinson.

[Extracted from a Communication made by Dr. Wilkinson to the Bath and West of England Society, and read at their Annual Meeting on the 19th of Dec. 1820.]

The manufacture of this acid is conducted on a large scale at Neath, in the neighbourhood of Swansea. The furnaces are made about 5 feet by 3, and 6 feet deep, sufficient to contain for each charge about 15 cwt. of wood; the door is made air tight by means of a luting of clay and horse dung, and is not opened for twelve hours; the fire underneath is raised just sufficient to produce a slight glowing heat on the floor of the furnace. All kinds of woods are made use of; the drier the wood, the stronger the acid. When the distillation is completed, what is left in the furnace is charcoal, which constitutes about one-third in weight of the wood employed; each ton of wood yields about 100 gallons of liquor, consisting of weak acid, tar, and naphtha, and the remaining loss arises from the gaseous products.

The acid corresponds in all its properties to acetic acid. After rest,

rest, a partial separation takes place; the denser tar falls to the bottom; the lighter naphtha floats at the top; the acid, the middle part, from which place it is drawn off: in this state it is a little mixed with portions of naphtha and tar, and is denominated by the manufacturer the black acid, for if it were in this state employed in the manufacture of sugar of lead, it would produce a very discoloured article. By distillation the acid becomes more concentrated and purified: it is in this stage that portion which is sold to the vinegar makers is saturated with chalk; the solution drawn off and evaported to dryness is exported in the form of the acetate of lime. For the manufacture of the acetate or sugar of lead, the acetate of lime is put into an iron still; sulphuric acid diluted with an equal quantity of water is added, in such a proportion as to leave an excess of sulphuric acid. The acetic acid is distilled, and the sulphate of lime or gypsum, formed in the operation, is left in the still.

The acid drawn off is subjected to another distillation; the acid in this state is highly concentrated, and the 100 gallons of liquor produced in the first distillation is reduced to about 30 gallons of strong acid. In this state it will dissolve nearly half its weight of litharge, which is added to the vinegar whilst cool; during its admixture there is an increase of temperature of near 60 degrees; in this stage no heat is employed. If the acid were warmed, there would not only be a considerable loss from evaporation; but the litharge instead of being dissolved would form a hard cake or mass. The manufacturer soon ascertains the point of saturation: he distinguishes by the smell an excess of either acid or litharge. If the mass is too thick to let the impurities subside, as much water is added as is equal to the acid employed; after being well stirred it is left for 24 hours to depurate, then drawn off and boiled down to concentration, which is determined by taking out a small portion in a capsule, and observing whether it solidifies on becoming cool. It is then drawn off into casks holding about 6 or 7 cwt. In about five or six days it becomes solid, at the end of which time, a hole is made at the top, into which a syphon is introduced, in order to draw off the mother-water from the central part; it is then broke up, picked, and sorted for the calico printers.

There being no correct account of this process published, will I hope plead my apology in troubling the Society with the above detail.

From some experiments which have been made, the purified acid has not been found to answer the same purposes as vinegar in the processes of pickling vegetable matter; in its pure state it is so highly concentrated, as to completely decompose onions, cucumbers, &c.; nor will it answer by reducing it by admixture

of water. I presume that the principle of pickling vegetable, or of salting or preserving animal matter, is to remove all the aqueous portion, and to prevent any subsequent access to that fluid. No decomposition is effected without water; the separation of the elementary principles of vegetable and animal matter is the combined result of the chemical agency of their constituent particles and those of water; if the vegetable matter, or the animal fibre, be in contact with such substances as will not be decomposed, or admit of the accession of water, then the animal and vegetable matter will not be subjected to any destruction.

Upon these principles I presume we may explain why pure acid, reduced by dilution with water, will not answer for pickling: as the common vinegar of the shops will bear with advantage an increase of strength, this is profitably effected by a proportionate admixture with this acid.

After troubling the Society with the above remarks, I shall now beg leave to direct their attention to the more important observations and experiments of Mr. Sockett. This gentleman, having directed his attention to the smoking of hams with wood smoke, either in a building erected for that purpose, or in a chimney where wood alone is burned, in addition to its considerable increase of flavour, he considered it more effectually preserved from putrefaction by being, what is commonly called, smoke-dried. Mr. S. having ascertained by experiments that meat thus cured required less salt, he was induced to suppose some antiseptic quality in the same, and not attributable to the mere application of heat. A neighbouring manufactory of pyroligneous acid afforded him an opportunity of trying a variety of experiments, which convinced him of the correctness of the supposition of the antiseptic quality of wood smoke, as the same effects as to flavour and preservation were produced in a superior degree without the aid of any increase of temperature, which by drying diminishes the nutritious quality of meat thus exposed.

Mr. S. ascertained that if a ham had the reduced quantity of salt usually employed for smoke-dried hams, and was then exposed to smoke, putrefaction soon took place when pyroligneous acid was not used; even one half this reduced portion of salt is sufficient when it is used, being applied cold, and the ham is thus effectually cured without any loss of weight, and retaining more animal juices.

The mode adopted was by mixing about two table spoonfuls of the acid, the same as here sent, in the pickle for a ham of 10 or 12 lbs.; and when taken out of the pickle, previous to being hung up, painted over with the acid by means of a brush. In many instances, Mr. S. has succeeded by brushing the ham over with the acid, without adding any to the pickle. The same mode

answers

answers equally well with tongues, requiring a little more acid, on account of the thickness and hardness of the integuments.

Upon dried salmon it answers admirably; brushing it over once or twice had a better effect than two months' smoking in the usual way, and without the same loss from rancidity.—From the result of a few experiments on herrings, he is persuaded that this mode of curing might be most advantageously introduced in our fisheries, so that herrings might be cured here superior to those imported from Holland.

These experiments so satisfactorily demonstrating the antiseptic qualities of this acid, where only small portions of salt were employed, Mr. S. was then induced to try the results of the application of this acid when no salt was employed: he placed some beef steaks upon a plate, and covered the bottom with the acid, the steaks being daily turned; and at the time of recording the experiment, he noticed that they kept above six weeks without the least tendency to putrefaction: this experiment was made in the middle of July 1815.

The first experiments reported by Mr. Sockett were made at the commencement of the year 1815, nearly six years since. Not only Mr. S. but many families in Swansea and its vicinity practise with the greatest success this mode of curing hams, tongues, beef, fish, &c.

Within these two or three years, paragraphs have appeared in different periodical works, published on the continent, as well as in this kingdom, testifying the antiseptic qualities af the pyroligneous or wood acid. I have no recollection of any observation on such an application of this acid, anterior to the experiments of Mr. Sockett. This acid is very easily and cheaply prepared: the first distilled product of the wood, in that state denominated black acid, answers the best when separated from its tar and naphtha. More than 70 gallons of acid, sufficiently strong, are procured from a ton of wood: this quantity of wood is readily procured at 10*s.* a ton, delivered at the works. Supposing the expenses of the manufacture to be equal to the purchase of the wood (and which is making a very ample allowance, as the residual charcoal is equal in value to the wood employed), the value of 70 gallons of acid will not exceed 20*s.* or about 3½*d.* a gallon: a gallon is quite sufficient for 2½ cwt. of pork, beef, and most animal substances, with the addition of a comparatively small portion of salt, not only affording a considerable saving in this article, but also materially contributing to the increase of flavour and nutritive quality. Hams or beef cured this way require no previous soaking in water to being boiled, and when boiled swell in size and are extremely succulent.

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The ham sent for the Society's examination has been cured in the usual way recommended for Westphalia hams; the acid was employed, instead of being smoked, two table spoonfuls of the acid being added to the pickle; and when the ham is to be removed from the pickle, it must be well washed in cold spring water and dried, and then some of the acid applied over it by means of a brush, and this repeated two or three times at about a week's interval.

Herrings Mr. S. cures with very little salt. Being well dried, as early after being caught as can be effected, they are then dipped into a vat of the acid, and when dry, the same process repeated a few times, suspending them like the manufacture of candles. Mr. S. entertains no doubt, from the result of his experiments with herrings, that the same process would answer for other kinds of fish, as salmon, cod, &c.; and hence, when cooked, may be salted according to each individual's taste. The red colour, in dried salmon and herrings, has been generally attributed to nitre; very frequently tobacco, dissolved in a fluid not very agreeable (urine), is made use of in Holland.

I presume this acid would be found very useful on board any vessel fitted out for long voyages; it appears from calculations on a small scale, that one hogshead of this acid would suffice to cure six tons of fish, in such a manner as to retain their nutritious quality; and they could be cured on board when opportunities occurred of procuring them, independent of its being an excellent substitute for common vinegar in many culinary purposes on board.

At the next meeting of the Society, I indulge a hope that I shall be enabled to lay before them a memoir containing a detail of experiments, in which I am at present engaged, to counteract the effects which are experienced in the vicinity of copper and lead works. I do not believe that any of the western counties are subjected to the melancholy influence of such smelting manufactories. In different parts of Wales, Derbyshire, Northumberland and Yorkshire, many thousand acres of land are nearly rendered incapable of producing vegetable matter, and the little pasturage they do afford proves very destructive to any animals feeding thereupon. In the smelting houses below Swansea and Neath, the atmosphere is obscured by clouds of smoke, highly charged with sulphureous and arsenious acid; which, when precipitated on fields, where cattle are feeding, produce such effects as might be expected from the agency of such poisons inducing large swellings on their joints, a complete change in the form of their feet, which must arise from local applications; also all the teeth, instead of being perpendicular to the jaws, are brought

into

into an horizontal direction. I flatter myself that I shall be able to suggest such alterations in the process, as will prevent the diffusion of the noxious poisons.

N. B. Mr. S. recommends that fish, as soon as practicable after taken, should be a little rubbed with salt, and laid upon a sloping board to drain, and when dry to be dipped in the acid as before stated. The ham sent has been cured a year, and not kept in a place very favourable for preservation. It must not be soaked in water, previous to being cooked. One great advantage attending this mode of curing hams or beef is, that when hung up they are never attacked by the flies.

III. *On the Magnitude of the Year.* *By Mr.* Yeates. *To Mr. Tilloch.*

Sir,—The space of time between the sun's passage from one solstitial point to the same again, is called a tropical or solar year, and also the space of time between the sun's passage from one equinoctial point to the same again; which year has a regard to the seasons, and marks their annual progress, and return:—and the space of time elapsed between the sun's apparent motion from one fixed star to the same star again, is called a sidereal year.

The ancient astronomers did not distinguish the tropical from the sidereal year with any precision until the time of Hipparchus, who is said first to have discovered the sidereal year to be greater than the solar year; and who concluded from thence, that the stars had a slow annual motion of their own from west to east. This discovery laid the foundation for the doctrine of the precession of the equinoxes, which is one of the profoundest parts of astronomy.

Copernicus relates two most ancient observations on the magnitude of the solar year at many years distance; the one by Hipparchus in the 177th Egyptian year after the death of Alexander the Great, and the other made by Ptolemy in the 463rd Egyptian year after; which interval Ptolemy computed at 285 Egyptian years, 70 days, 7 hours, and 12 minutes; in which space of time his commentator reckons just 285 tropical years, where an Egyptian year contains twelve months of thirty days each, and five intercalary days, and where both the Egyptian year and day begin at noon; so that dividing 70 days, 7 hours, 12 minutes, by 285, the quotient is 5 hours, 55 minutes, 12 seconds, and the solar or tropical year given at 365 days, 5 hours, 55 minutes, 12 seconds. After this method used by Ptolemy, astronomers having made accurate observations of the equinoxes, and computing the same at many years distances,

stance, have approximated to a still greater degree of exactness. Tycho Brahe by this method computed the solar tropical year at 365 days, 5 hours, 49 minutes; and the same measure was found on examining two corresponding observations at 168 years distance; the one by Tycho Brahe at Uraniberg, A.D. 1585, and the other by Dr. Bradley, at Greenwich, 1753.

Thebites, an Arabian astronomer, about the year of Christ 1200, computed the sidereal year at 365 days, 6 hours, 9 minutes, 12 seconds; an exactness confirmed by the most able and accurate astronomers of late years, aided by the most superior and perfect instruments fabricated by human hands. This measure very nearly agrees with that of the Indian astronomers, computed at 365 days, 6 hours, 12 minutes, and 9 seconds; see Phil. Mag. vol. 55. p. 314. The Newtonian astronomy reckons 365 days, 6 hours, 9 minutes. These are evident proofs of the fact, that the sidereal year is greater than the solar year, as attested by astronomers of different nations.

It is generally believed the Hebrews and Egyptians followed the reckoning of the antediluvians in their years and months; each month consisting of 30 days, and each year of twelve months and five intercalary days, in respect of the sun; and therefore it is that Hermes Trismegistus, the earliest of the Egyptian astronomers, fixed the year at 365 days, which is the common quantity in whole days; but this not being found to correspond with the solar motions, Ennius, another astronomer of the same nation, stated the full year of the sun at 366 days, which quantity Democritus, who studied among the Chaldeans, Calippus, Archimedes, Gemins of Rhodes, and Sosigenes, who assisted Julius Cæsar in reforming the Roman calendar, also followed; and hence we must consider the solar year to have been computed in three several quantities, the common year 365 days, the full year 366 days, and the mean or astronomical year at 365 days, 6 hours.

The year of Oenepidus of Chios, computed at 365 days, 8 hours, 57 minutes; that of Harpalus, at 365 days, 13 hours; and that of Meton, at 365 days, 6 hours, 18 minutes, 56 seconds, belongs to the stars, wherein Meton, the inventor of the lunar cycle among the Greeks, came very near the truth.

Hipparchus, the first among the ancients who discovered the motion of the equinoxes, was the first who determined the quantity of the solar year with any accuracy; he was followed by Ptolemy, and Rabbi Adda the Jew, who corrected the Hebrew calendar about A.D. 340, and others; and this quantity discovered by Hipparchus is the nearest and most convenient of any other for the joint motions of the sun and moon. I shall here put down the several quantities deduced from his principle:

1. Hip-

	D.	H.	M.	"	
1. Hipparchus	365	5	55	16	
2. Ptolemy	365	5	55	12	
3. R. Adda	365	5	55	25	26'' 20'''
4. Copernicus' greatest magnitude	365	5	55	37	4
5. Prutenic Tables, ditto ..	365	5	55	53	
6. Chr. Sev. Longomontanus, ditto	365	5	51	29	12
7. David Organus, ditto ..	365	5	56	53	

The Copernicans distinguished a greatest, mean, and least measure for the solar year, according to which the solar years are either of various quantities, or else no certain quantity was certainly discoverable by them common to them all.

It is well known that the lunar year makes an entire revolution, and moves through all the seasons, in 33 years; in like manner the solar year is supposed to revolve at the extremely slow progress of 50'' per annum, and in 25412 years completing its motion through the great circle of the heavens; and this opinion still prevails in the modern astronomy, and will until the theory of the celestial motions be better understood.

The Julian calendar preserves a mean between the solar and sidereal year; and hence it is that this calendar is made the standard measure of time among astronomers; so that by suppressing a certain number of days in a long period of years, the solar year is regulated by the seasons; and by adding a certain number of days the sidereal years are conveniently ascertained, which regulation of time constitutes the principle of the Gregorian calendar.

The Julian calendar constantly adds one day in four years; and because this reckoning in a great number of years is not found to keep to the seasons, therefore the Gregorian calendar suppresses three days in four hundred years, that so the equinoctial days in the calendar may fall on the true equinoctial days of the sun.

In 912 Julian years are 47586 weeks, and 6 days; and in 912 solar years are 47585 weeks, and 6 days: so that the difference between the calendar Julian and Gregorian account is one week. In this period one minute less or more in the quantity of a solar year will make a difference of fifteen hours, twelve minutes; and one second, less or more, will amount to fifteen minutes, twelve seconds; according to which the following table is constructed for the various quantities assigned for the solar year, and the anticipation of time in days for 912 years.

	D.	H.	M.	Calendar. Days suppressed.
Calippus.	365	6	00	
	365	5	59	0 days 16 hours.
	365	5	58	1 day 7 hours.
	365	5	57	1 day 22 hours.
	365	5	56	2 days 13 hours.
Hipparchus.	365	5	55	3 days 4 hours.
	365	5	54	3 days 20 hours.
	365	5	53	4 days 11 hours.
	365	5	52	5 days 2 hours.
	365	5	51	5 days 17 hours.
	365	5	50	6 days 8 hours.
Tycho Brahe.	365	5	49	7 days.
	365	5	48	7 days 15 hours.
	365	5	47	8 days 6 hours.
	365	5	46	8 days 22 hours.
	365	5	45	9 days 12 hours.
	365	5	44	10 days 4 hours.
	365	5	43	10 days 19 hours.
	365	5	42	11 days 10 hours.

Remark: "Astronomers having divided the interval of the several returns of the sun to the same colure, by the number of revolutions, have found that the revolution was of 365 days 5^h 48′ 48″, less by 20′ than that observed in respect of the stars; whence they have concluded, that each colure retrograded 49 or 50″ every year. They have therefore called the *Tropical* year, that whose revolution is of 365 days 5^h 49′; and the *Sidereal* year, that which is made in 365 days 6^h 9 min."—*La Caille's Elements*, translated by Robertson, art. 471.

The subjoined list is forwarded for insertion, if it will add to the advantage of useful investigation; and I wish, sir, some of your able correspondents would furnish a like list of such various measures of the solar, sidereal and lunar year, as have been determined on by modern astronomers in England and other parts of Europe. Sir, yours respectfully,

T. Yeates.

The various Magnitudes of the Year as determined by the ancient Astronomers.

		D.	H.	M.
Hermes Trismegistus, an Egyptian astronomer . .	An. an. Chr. 1480	365	0	0
Ennius, an Egyptian astronomer,	An. an. Chr. 700	366	0	0
Thales the Milesian, a Grecian astronomer	An. an. Chr. 620	365	0	0
Denepidus of Chios, a Grecian astronomer	An. an. Chr. 560	365	8	57

Harpalus

		D.	H.	M.	
Harpalus	An. an. Chr. 520	365	13	0	
Democritus of Abdera, who studied among the Chaldeans	An. an. Chr. 456	365	6	0	
Meton, the inventor of the lunar Cycle, who with Eutemon observed the solstices . . .	An. an. Chr. 422	365	6	18	56″ 51‴
Aphroditus, an Egyptian . .	An. an. Chr. 400	365	3	0	
Calippus Cyzicenes	An. an. Chr. 330	365	6	0	
Aristarchus of Samos . . .	An. an. Chr. 282	365	6	1	16″
Archimedes of Sicily . . .	An. an. Chr. 266	365	6	0	
Hipparchus the Bythinian . .	An. an. Chr. 136	365	5	055	16″
Genius of Rhodes	An. an. Chr. 83	365	6	0	
Sosigenes, who assisted Julius Cæsar in reforming the Calendar	An. an. Chr. 46	365	6	0	
Isaacius, a Jewish priest . .	An. Chr. (uncert.)	365	5	8	
Ptolemy	An. Chr. 140	365	5	55	12″
Rabbi Adda the Jew, who corrected the Hebrew Calendar	An. Chr. 340	365	5	55	25″ 26‴ 20⁗
Rabbi Samuel	(uncertain)	365	6	0	
Albategnius in Assyria	879	365	5	46′	24″
The Persian astronomers, according to Scaliger .		365	5	48	30
Longomontanus		365	5	48	53 20
Bulialdus		365	5	48	59
Alphonsus, king of Castile, assisted by the Arabian, Moorish and Jewish astronomers	1250	365	5	49	15″ 58‴ 49⁗ [16′′′′′ 26′′′′′′
This magnitude of the year was followed by Georgius Perbachus at Ferrara, in Italy and Vienna	1450	365	5	49	16″
Bernardus Waltherus of Nuremburg, a scholar of Regiomontanus	1504	365	5	48	50
Copernicus, three magnitudes:					
Greatest 365 5 55 37 4; Least . 365 5 42 55 7	Mean	365	5	49	16 23 30
Hieronymus Cardan, Professor at Bononia	1530	365	5	48	41
Daniel Lambech	1561	365	5	48	41″ 42‴ [33⁗ 50′′′′′ 10′′′′′′
Ignatius Danter of Bononia	1576	365	5	45	36
Maginus, from the Prutenic Tables, three magnitudes:					
Greatest 365 5 55 53; Least . 365 5 42 38	Mean	365	5	49	16
Meesthines from the same Tables		365	5	49	15 46
Christophorus Clavius and Aloysius Lelius at the request of Pope Gregory XIII.	1582	365	5	49	12
Tycho Brahe the Danish astronomer		365	5	48	45
Kepler of Wirtemburg, mathematician to three Emperors	1627	365	5	48	57 36
Christianus Severini Longomontanus, Danish Professor at Hafnia or Copenhagen: Three magnitudes:					
Greatest 365 5 51 29 12; Least . 365 5 46 20 48	Mean	365	5	48	55
David Organus of Silesia, Professor of Ma-					

thematics at Frankfort, in his Ephemerides of 59 years, or from An. 1635 to 1694:

Three magnitudes:

Greatest		365	5 56 53		
Least .		365	5 42 38	Mean 365	5 49 15
Galifredus Vendelinus, a Dutchman . . .	1644	365	5 49	5 27 16	
Ismael Bulialdus	1645	365	5 49	4 21 3	
Johannes Baptista of Bononia	1651	365	5 48 48		
Philippus Lansbergius of Ghent	1682	365	5 48′ 57″ 2‴ [22⁗ 4′′′′′		
Dionysius Petavius, *De Ratione Tempor.* . . .		365	5 49		
Dr. Gregory in his Astronomy		365	5 49		
Cassini, Dr. Keil, &c.		365	5 48 57		

IV. *Account of the Voyage of Discovery and Circumnavigation performed in* 1818, 1819, *and* 1820, *by Capt.* Freycinet, *Commander of the French Corvette* Urania*.

M. Louis de Freycinet, captain of a frigate, to whom the King had intrusted the command of the corvette Urania, in order to make a voyage of discoveries in the South Seas, arrived at Havre on the 13th of November 1820.

The principal object of this expedition was to make the necessary observations for determining the configuration of the earth, and the strength of the magnetic power in the southern hemisphere; but having to traverse, during more than two years, a great extent of sea, M. de Freycinet was also to take advantage of all occasions which might offer to him, to augment our collections of natural history, to add new documents in hydrography to those which are already deposited in the Royal Marine depôt.

The corvette Urania, fitted out at Toulon in the early part of 1817, was furnished with every article necessary for a long voyage; she received a picked crew, and her quarter-deck was composed of officers equally distinguished for their zeal and the extent of their knowledge.

A numerous collection of the best instruments for physical and nautical astronomy were put on board, to be used in the experiments and observations which were the essential objects of the voyage.

The Royal Academy of Sciences anxiously drew up, for M. de Freycinet, notes necessary to guide him in his researches into general physics, natural history, geology, mineralogy, &c.

After long delays, occasioned by the difficulty of getting on board different objects necessary for the undertaking, the Urania set sail on the 17th of September 1817.

Contrary winds obliged them to put into Gibraltar on the 11th

* From the *Moniteur*.

of

of October, and she did not arrive at Santa Crus, in the island of Teneriffe, before the 22d of the same month.

This port would have been a commodious place for making observations of various kinds; but the necessity of first submitting to a long quarantine, determined M. de Freycinet to stop only for six days; and on the 28th of October he sailed for the Brasils.

On the 6th day of December Cape Frio was observed, and its geographical position verified. The Urania entered Rio de Janeiro the same night, where she remained until the 29th of January.

This stay of nearly two months was not so usefully employed as M. de Freycinet wished. Some difficulties at first opposed themselves to the establishment of an observatory on shore. The bad weather, too, obstructed the astronomical observations; but those in magnetism, and the oscillations of the pendulum, were made with the greatest care; and at the same time the numerous specimens of natural history and drawings of all kinds commenced the valuable collections which were to be the fruits of the expedition.

The passage from Rio Janeiro to the Cape of Good Hope was marked by a melancholy event, which deprived M. de Freycinet of one of his ablest colleages. M. Laborde, an officer of distinguished merit; an accurate observer; a good draughtsman, and who joined to these excellent qualities a character the most sociable, died in the flower of his age. His loss at first caused an universal sorrow.

The Urania remained in Table Bay from the 7th of March till the 5th of April; and from thence they sailed to Port Louis, in the Isle of France, where they arrived on the 5th of May.

M. de Freycinet praises particularly the reception which he met with during these two stoppages from Lord C. Somerset, the Governor of the Cape; and from Mr. G. Smith, Chief Judge and Commissioner of Justice at Port Louis, from whom he received the greatest facilities, as well for the establishment of his observatory a-shore, as for the advancement of every thing which could contribute to the success of his mission.

Port Louis, placed nearly in the same latitude as Rio de Janeiro, and at a distance of more than 100 degrees in longitude, was favourably situated for observations respecting the pendulum. Those were made in detail, as well as experiments, the objects of which were to enlarge the study of magnetism, and of meteorology.

A very considerable damage, which had torn off the copper sheathing of the Urania, did not allow them to put to sea until the 16th of July. The corvette stopped only some days at the

Isle

Isle of Bourbon to take in provisions, and then directed her course towards the coasts of New Holland, the northern extremity of which was seen on the 11th of September 1818. (This part of the coast is called Edel's Land.)

The Urania coasted along at a moderate distance, and, having fallen in with Endracht's Land, she followed it until she arrived at the entrance of Sea Dog's Bay, from whence, after a short stay, she sailed on the 13th of September to the anchorage before the peninsula of Peron.

An observatory was at first established on shore, and then they were employed in procuring, by means of distillation, water fit to be drunk. Two stills had been shipped at Toulon for this purpose. Numerous defects, which it may probably be easy to remedy in other vessels, rendered almost null the products of the apparatus placed on board the corvette; but that which was put up on shore gave, in sufficient abundance, water pleasant to drink, and in which they could discover no noxious quality.

The Urania sailed on the 26th of September; the intention of M. de Freycinet being to sail for Timor, in order to ascertain some points respecting its geographical position, of which he had doubts. He consequently sailed near the Isles of Dorre and Bernier, which he coasted along at a good distance to the eastward, and in shallow water; when the corvette having struck on a sand bank, he was obliged to abandon the labour begun, and to bear off from the shore.

This event had no disagreeable consequence; the time passed at the anchorage on the bank was employed in exploring its figure and soundings; and M. de Freycinet gave it the name of the Bank of Urania.

On the 29th of October 1820, the corvette cast anchor in the Bay of Coupang, in the Island of Timor, after having coasted on the west side of the Isles of Limas and Retti, which belong to that archipelago.

The inhabitants of Coupang were then only busied in preparations for the war which the Dutch Government was going to make on the Rajah, Louis d'Amanoebang.

This circumstance rendered it difficult to purchase the provisions necessary to victual the corvette; but it did not hinder the scientific operations, which were carried on with the greatest zeal, in spite of the excessive height of the temperature; at the Observatory it stood, at times, at 45 degrees of the thermometer (Reaumur's); whilst in the shade it kept at 33 or 35 degrees.

The Urania sailed from Coupang on the 23d of October 1818, very badly provisioned, and with several men attacked with dysentery.

Calms and contrary currents detained them a long time between

tween Timor and Ombay. This was taken advantage of to visit the village of Bitoca; it is situated on the south coast of the latter of these islands; has been, till now, little frequented by Europeans, and is peopled by a warlike and ferocious race, some of whom are anthropophagites.

Meanwhile, the number of dysenteric patients increased on board the corvette, and all the skill of M. Quoy, the surgeon-major, was not sufficient to overcome the influence of a devouring climate. The harbour of Coupang had furnished them with but few refreshments; it became, therefore, necessary to take a new station at Timor, and accordingly the Urania anchored at Diely, the chief place among the Portuguese establishments on the north coast of that island.

A most obliging reception was given to the Expedition by Don Jose Pinto Alcoforado d'Azevedo e Souza; and the corvette was abundantly provisioned, through his care, with every thing that she wanted.

Their stay here was only for five days, after which the Urania bent her course still along the coast of Timor, in order to get through the Straits to the eastward of Vitters, by the channel that separates that isle from those of Kiffer and Roma.

On the 29th of November they were in sight of Ceram and Amboyna, and stretching into the Strait between the latter island and Bonnon, they bent their course towards the Isle Gasse, which they doubled to the eastward at a small distance, during a violent storm. A great number of isles were observed, among which the most remarkable are those of Damoner, Gilolo, and Guébé.

In this passage the Urania fell in with several armed canoes belonging to the Kimalaha of Guébé. This Prince came on board, and passed an entire day with them, during which his flotilla towed astern of the corvette. He furnished M. de Freycinet with various information respecting his country and his maritime expeditions, and made the strongest endeavours to induce him to stop at his island, where he assured him there was an excellent harbour, a commodious watering-place, and good refreshments. This proposition not being accepted, he assured him he would come with his brothers to Waigiou, and pay him a new visit.

It was to the Isle Guébé that M. de Pavre was sent formerly by M. de Coëtiva to take drawings of the nutmeg trees which have since multiplied so much in the Indian and American colonies. The Guébéans recollected that circumstance very well, of which they were themselves the first to speak; and M. de Freycinet attributes to their former relations with the French, the very particular amity which they testified towards him.

A pretty

A pretty fresh breeze put an end to these amicable communications. The Urania, continuing her track, passed, on the 12th of December, the strait which separates the Isle of Mouhox from Guébé, and stretched to the eastward; she ran some risk in the strait formed by the Isles of Rouib and of Balabalak, and by the Wyag Islands, where, during a calm, violent currents set upon shallows; but she was fortunately able to keep her anchorage, and to wait for such winds as permitted her to keep her way, until she had got clear of that perilous situation.

She cast anchor on the 16th of December, at the Isle of Rawak, after having at a short distance coasted along the northern side of Waigion.

An observatory was established on shore, and its position, in latitude only 1½ minute south, was the most favourable for experiments with the pendulum which they could get under the equator. The period of this stay was employed in researches respecting geography and natural history.

Two or three days before they sailed, they heard, on a sudden, the martial music of tomtoms, kettle-drums, &c. Some moments after, there appeared, at the large point of the island, the fleet of the Kimalaha of Guebe, who, faithful to his promise, had come to pay the visit he had before announced. This little squadron presented a spectacle at once imposing and whimsical. The Guebean Prince was accompanied by his brothers, and sons, to the number of eight; all, like himself, of good mien, and remarkable for their intelligence. They remained on board until the moment of the corvette's departure; they gave, as presents to M. de Freycinet, various curiosities of their country, and, among others, hats made of straw and isinglass (*talc*) worked with admirable art.

Having sailed from Rawak on the 5th of January 1819, the Urania stretched towards the Ayon isles, which they saw on the 6th and 8th of the same month.

The dysentery continued still to torment the crew; it was not long before it was joined to fevers, one of the first victims of which was M. Labiche, the second lieutenant, an officer full of merit, and of the most amiable character. This was the second loss of the kind during the voyage, and it was keenly felt.

After having visited several of the Caroline isles, which are not pointed out on the maps, and having received throughout the most friendly reception from the islanders, M. de Freycinet arrived on the 17th of May in sight of the Isle of Guam, and cast anchor on the night of the same day in the roadstead of Humata. This delay, and that which the corvette made at Port San Louis in the same island, restored health to the crew, thanks to the generous eagerness with which the governor, Don Jose de Medinillo

dinillo y Pineda, anticipated all the wants of the expedition, by procuring them refreshments and comforts of all kinds.

M. de Freycinet appears to have collected, respecting the people of the Marianne Islands, information more extensive than that with which preceding voyagers have enriched their accounts. He gives various details respecting their manners, language, and laws, as well as that singular government of which much has been said, and in which the women act an important part. He communicates to us interesting notions respecting the arts which they practise, respecting their money, which is established on principles absolutely different from ours, and respecting their architecture, of which he still saw numerous ruins at Tinian.

Two months were employed in making these researches; and at the same time they were occupied with those observations and experiments which formed the principal object of the expedition. M. de Medinillo had, during all this time, the kindness to provide the corvette abundantly with fresh provisions, to which he added provisions for the voyage, and for which he afterwards refused to accept any reimbursement.

The course of the Urania, from Guam to the Sandwich Islands, presents nothing remarkable. On the 5th of August 1819, she made the island of Owhyhee, and anchored in the bay of Harahona in three days after.

Tamahama, king of the Sandwich Isles, was dead; his palace had been reduced to ashes, and almost all the hogs on the island had been slaughtered on account of his obsequies, according to the custom of the country; which was a real disappointment in the re-victualling of the corvette.

Uno Rio, the eldest son and successor of Tamahama, enjoyed at that time but a badly-established authority. The chiefs compelled to submit to the arms of his father, raising extraordinary pretensions, caused him to dread an approaching war. He came with his wives and a numerous suite on board the Urania, on the occasion of the baptism of one of the principal chiefs of the island. That ceremony was performed with much pomp by the Abbé Quelen, chaplain of the vessel.

The Sandwich Islands were, like the Marianne, the object of the assiduous researches of M. de Freycinet and of the officers under his command. Numerous observations were made in search of the magnetic equator, and its inflexions, in the Great Ocean.

On the 30th of August the Urania sailed for Port Jackson, passing through the islands of the Austral Polynesia. By taking this track, the position of the dangerous isles of Byron was rectified,

ified, as well as that of the Island of Pyletant, the most southerly of the Friendly Islands; and also that of Howe Island.

A new island, surrounded by dangerous reefs, was discovered to the east of Tonga, which M. de Freycinet named Rose Island.

The Urania anchored in Port Jackson on the 18th of November 1819; she remained there till the 25th of December, and this interval was employed, as at all the preceding stoppages, in scientific inquiries. M. de Freycinet speaks in this respect with gratitude for the assistance afforded to him by Mr. Macquarie, the governor of the colony.

On quitting Port Jackson, the course of the corvette was shaped to pass between Van Diemen's Land and New Zealand. On the 7th of January 1820, the southern extremity of the latter islands was doubled in sight of Campbell's Island.

From that moment until nearing the coast of Terra del Fuego the winds were constantly favourable. The Urania reached 59 degrees of south latitude; and she found floating ice in the 54th degree.

On the 5th of February the coast of Terra del Fuego was seen in the neighbourhood of Cape Desolation; the season was as frightful as the adjoining shores. In the impossibility of reaching Christmas Harbour, it became necessary to make for the Bay of Good Success, in the Straits of Lemaire; but hardly had the anchor dropped, when a furious storm began to cause the corvette to drive: there was not a moment to be lost in cutting the cable, and setting sail with all speed, in order to get out of the Bay, by skirting at a very short distance the rocks and breakers which lie upon its north point.

This dreadful tempest lasted for two days, and made the corvette drift considerably to the northward; which determined M. de Freycinet to bear up for the Falkland Islands, in sight of which they arrived on the 14th of February, according to their reckoning, but the 13th according to European time, they having gained a day in circumnavigating the globe.

[The public are already acquainted with the loss of the Urania, in consequence of striking on a sunken rock, at the entrance of French Bay, in the Falkland Islands, and of their being taken off by an American whaler, and brought first to Rio Janeiro, and afterwards to Havre de Grace, where they arrived in safety, with most of the collections made during the voyage.]

In expectation that more detailed accounts (proceeds the narrative) will make known all the importance of their labours, it will suffice to give a rapid glance at them.

1st. The observations on the pendulum, which formed one of the principal objects of the voyage, have been made with the

greatest

greatest care at every place where they stopped, and in every situation throughout the voyage which would permit. The stations where these experiments were made are nine in number, viz. Rio Janeiro (first stay); the Cape of Good Hope; Port Louis, in the Isle of France; the Island of Rawak; the Island Guam; the Island of Mowa, in the Sandwich Isles; Port Jackson; the Falkland Islands; and at Rio Janeiro (second stay).

2d. Each day during the voyage, two officers at least took by rotation the necessary astronomical observations to ascertain the situation of the vessel at sea, and, on shore, the positions of the different observatories; to regulate the chronometers, &c. All these observations have been transcribed into journals destined for that purpose.

3d. The magnetic phænomena were at the same time the object of constant and multiplied studies, as well at sea as in all the places which they touched it. They comprise observations on the magnetic declination and inclination; on the intensity of both when tried by the horizontal needle, or the needle of inclination; and also on the hourly and periodical variations in the declination.

4th. Comparative observations on the temperature of the air, with that of the sea at its surface, were made every two hours during the whole course of the voyage. This considerable mass of results may be useful to determine the isothermic lines on the terrestrial globe.

5th. More than 60 specimens of sea-water, taken in the seas which they traversed, were put into as many flasks, perfectly sealed up, in order to be analysed on their return. Each flask was labelled with the latitude and longitude of the spot where the water was drawn.

6th. A meteorological journal kept every hour during the whole voyage, will show in methodical order all the observations on the thermometer, the barometer, and the hydrometer, which they made both by sea and land. They will also show the indications of the prevailing winds, and their degrees of force, the electrical and aërial phænomena, &c.

7th. The barometrical variations could not be observed with precision except in the places which they touched at. The results of them have been consigned to a particular register.

8th. It was not possible to observe the tides and currents, except at a small number of points; but the data acquired at Rio Janeiro, at the Isle of France, at Rawak, and at Guam, are not without interest.

9th. The number of charts formed during the voyage is about 30. A part of them have already been completed; but the whole of the materials collected on this subject, and classed

with great care, will give every facility desirable for carrying on this work.

10th. Notwithstanding the shipwreck at the Malouin or Falkland Islands, which caused the loss of 18 cases of specimens of natural history, there remain still about 40. These contain a great number of specimens out of the three kingdoms of nature; and especially almost the whole of those which were collected at the Marianne Islands, yet little known in that respect to naturalists.

11th. The number of drawings made during the voyage amounts to several hundreds; the greater part admirable for the beauty of the situations which they represent, or for the correctness of the portraits, and the graces of their composition.

12th. In short, the observations on the manners and customs of the people whom they visited, have been collected in very great number by all the officers employed in the expedition. All of them have been drawn up in the same spirit, and after the same plan, in order that they may connect themselves easily with the general account of the voyage.

It is above all to be remarked, that this is the first expedition of the same kind, in which all the scientific operations have been performed entirely by officers attached to the service of the Royal Marine of France.

V. *A Table of the Sun's Right Ascension to every Ten Minutes of his Longitude, with the Differences and Secular Variation for January* 1, 1801. (*Obliq. Eclip.* 23° 27′ 57″, *and Sec. Var.* 52″·1.) *By Mr.* JAMES UTTING, *Lynn Regis.*

To Mr. Tilloch.

SIR,—I SEND you for insertion in your Philosophical Magazine and Journal (if approved) Tables of the Sun's Right Ascension in degrees, &c., also in hours, minutes, &c. of the Sun's Declination, and also of the Reduction of the Ecliptic to the Equator. The whole are calculated from Taylor's Tables of Logarithms to Seconds, and will, I conceive, be found useful to such persons at least as do not possess the above tables.

I am, sir, yours truly,

Lynn Regis, Jan. 5, 1820. JAMES UTTING.

*** The first of the Tables sent by Mr. Utting is subjoined. The other three will appear in our following Numbers.—EDIT.

Argu-

Argument	Signs 0 and VI.			Signs I. and VII.			Signs II. and VIII.			Argument
☉'s Long.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	☉'s Long.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
0 0	0 0 0·00		0·00	27 54 20·84		9·35	57 48 48·76		10·19	30 0
10	0 9 10·38	550·38	0·06	28 3 54·06	573·22	9·39	57 59 13·57	624·81	10·16	50
20	0 18 20·76	550·38	0·12	28 13 27·51	573·45	9·42	58 9 38·68	625·11	10·13	40
30	0 27 31·14	550·38	0·18	28 23 1·20	573·69	9·46	58 20 4·06	625·38	10·10	30
40	0 36 41·53	550·39	0·24	28 32 35·14	573·94	9·49	58 30 29·70	625·64	10·07	20
50	0 45 51·92	550·39	0·30	28 42 9·32	574·18	9·52	58 40 55·64	625·94	10·04	10
1 00	0 55 2·32	550·40	0·36	28 51 43·72	574·40	9·56	58 51 21·85	626·21	10·01	29 0
10	1 4 12·73	550·41	0·42	29 1 18·40	574·68	9·59	59 1 48·33	626·48	9·97	50
20	1 13 23·16	550·43	0·48	29 10 53·32	574·92	9·63	59 12 15·10	626·77	9 94	40
30	1 22 33·59	550·43	0·54	29 20 28·47	575·15	9·66	59 22 42·17	627 07	9·91	30
40	1 31 44·03	550·44	0·60	29 30 3·88	575·41	9·69	59 33 9·48	627·31	9·88	20
50	1 40 54·49	550·46	0·66	29 39 39·54	575·66	9·72	59 43 37·08	627.60	9·84	10
2 00	1 50 4·97	550·48	0·72	29 49 15·43	575·89	9·75	59 54 4·94	627·86	9·81	28 0
10	1 59 15·46	550·49	0·78	29 58 51·59	576·16	9·79	60 4 33·10	628·16	9·77	50
20	2 8 25·97	550·51	0·85	30 8 28·00	576·41	9·82	60 15 1·51	628·41	9·74	40
30	2 17 36·51	550·54	0·91	30 18 4·65	576·65	9·85	60 25 30·20	628·69	9·71	30
40	2 26 47·06	550·55	0·97	30 27 41·56	576·91	9·88	60 35 59·18	628·98	9·67	20
50	2 35 57·64	550·58	1·03	30 37 18·71	577·15	9·91	60 46 28·42	629·24	9·64	10
3 00	2 45 8·25	550·61	1·09	30 46 56·14	577·43	9·94	60 56 57·92	629·50	9·60	27 0
10	2 54 18·88	550·63	1·14	30 56 33·81	577·67	9·97	61 7 27·69	629·77	9·56	50
20	3 3 29·54	550·66	1·20	31 6 11·53	577·72	10·00	61 17 57·74	630·05	9·53	40
30	3 12 40·23	550·69	1·26	31 15 49·92	578·39	10·03	61 28 28·04	630·30	9 49	30
40	3 21 50·95	550·72	1·32	31 25 28·38	578·46	10·06	61 38 58·63	630·59	9·45	20
50	3 31 1·70	550·75	1·38	31 35 7·08	578·70	10·08	61 49 29·46	630·83	9·42	10
4 00	3 40 12·48	550·78	1·44	31 44 46·02	578·94	10·11	62 0 0·57	631·11	9·38	26 0
10	3 49 23·31	550·83	1·50	31 54 25·26	579·24	10·14	62 10 31·94	631·37	9·34	50
20	3 58 34·17	550·86	1·56	32 4 4·74	579·48	10 17	62 21 3·57	631·63	9·30	40
30	4 7 45·06	550·89	1·62	32 13 44·47	579·73	10·20	62 31 35·45	631·88	9·26	30
40	4 16 56·00	550·94	1·68	32 23 24·49	580·02	10·22	62 42 7·62	632·17	9·22	20
50	4 26 6·98	550·98	1·74	32 33 4·74	580·25	10·25	62 52 40·02	632·40	9·18	10
5 00	4 35 18·00	551·02	1·80	32 42 45·30	580·56	10·28	63 3 12·69	632·67	9·14	25 0
10	4 44 29·06	551·06	1·86	32 52 26·09	580·79	10·30	63 13 45·60	632·91	9·09	50
20	4 53 40·17	551·11	1·92	33 2 7·17	581·08	10·33	63 24 18·79	633·19	9·05	40
30	5 2 51·33	551·16	1·98	33 11 48·50	581·33	10·36	63 34 52·23	633·44	9·01	30
40	5 12 2·54	551·21	2·04	33 21 30·13	581·63	10·38	63 45 25·92	633·69	8·97	20
50	5 21 13·80	551·26	2·10	33 31 12·00	581·87	10·41	63 55 59·85	633·93	8·92	10
6 00	5 30 25·10	551·30	2·16	33 40 54·11	582·11	10·44	64 6 34·06	634·21	8·88	24 0
10	5 39 36·46	551·36	2·22	33 50 36·54	582·43	10·46	64 17 8·50	634·44	8·84	50
20	5 48 47·88	551·42	2·28	34 0 19·22	582·68	10·48	64 27 43·19	634·69	8·79	40
30	5 57 59·35	551·47	2·34	34 10 2·18	582·96	10·51	64 38 18·13	634·94	8·75	30
40	6 7 10·87	551·52	2·40	34 19 45·39	583·21	10·53	64 48 53·31	635·18	8·70	20
50	6 16 22·46	551·59	2·46	34 29 28·91	583·52	10·55	64 59 28·76	635·45	8·66	10
7 00	6 25 34·11	551·65	2·52	34 39 12·69	583·78	10·57	65 10 4·45	635·69	8·61	23 0
10	6 34 45·82	551·71	2·58	34 48 56·73	584·04	10·60	65 20 40·36	635·91	8·57	50
20	6 43 57·59	551·77	2·64	34 58 41·07	584·34	10·62	65 31 16·54	636·18	8·52	40
30	6 53 9·43	551·84	2·69	35 8 25·67	584·60	10·64	65 41 52·95	636·41	8·48	30
40	7 2 21·33	551·90	2·75	35 18 10·53	584·86	10·66	65 52 29·59	636·64	8·43	20
50	7 11 33·30	551·97	2·81	35 27 55·70	585·17	10·68	66 3 6·47	636·88	8·39	10
8 00	7 20 45·34	552·04	2·87	35 37 41·11	585·41	10·70	66 13 43·59	637·12	8·34	22 0
		552·11			585·71			637·38		
	Signs V. and XI.			Signs IV. and X.			Signs III. and IX.			

TABLE continued

Argument ☾'s Long	Signs 0 and VI R A	Diff	S V	Signs I and VII R A	Diff	S V	Signs II and VIII R A	Diff	Sec Var	Argument ☽'s Long
° ′	° ′ ″	″	′ ″	° ′ ″	″		° ′ ″	″	″	° ′
10	7 29 57 15	552 18	2 2	[illegible] 47 26 82	586 02	10 72	66 24 29 97	637 59	8 29	50
20	7 39 9 63	552 25	2 [illegible]	[illegible] 12 [illegible]	586 27	10 7	66 34 58 56	637 84	8 25	40
30	7 48 21 83	552 31	3 04	[illegible] 6 5 11	586 55	10 76	66 45 36 10	638 05	8 20	30
40	7 57 34 22	552 40	3 10	[illegible] 16 [illegible]	586 82	10 78	66 56 14 45	638 30	8 15	20
50	8 6 46 62	552 48	3 1	[illegible] 22	587 13	10 [illegible]	67 6 52 75	638 54	8 10	10
9 00	8 15 59 10		3 1	36 [illegible] 19 01		10 82	67 17 31 2		8 05	21 0
		552 56			587 30			638 73		
10	8 25 11 66	552 65	3 27	[illegible] 46 7 0	587 65	10 84	67 28 10 02	638 98	8 00	50
20	8 34 24 31	552 72	3 [illegible]	35 55 54 68	587 95	10 86	67 38 49 00	639 22	7 95	40
30	8 43 37 03	552 81	3 39	[illegible] 12 6	588 23	10 88	67 49 28 22	639 43	7 90	30
40	8 52 49 84	552 90	3 44	37 1 50 39	588 55	10 90	68 0 7 65	639 65	7 85	20
50	9 2 2 74	552 98	3 50	37 2 19 14	588 81	10 91	68 10 47 30	639 88	7 80	10
10 00	9 11 15 72		3 56	37 35 8 2		10 93	68 21 27 18		7 75	20 0
		553 06			589 11			640 10		
10	9 20 28 78	553 15	3 62	37 14 [illegible] 36	589 41	10 9	68 32 7 28	640 32	7 70	50
20	9 29 41 93	553 25	3 67	37 54 46 77	589 [illegible]	10 96	68 42 47 60	640 53	7 65	40
30	9 38 55 18	553 34	3 73	38 4 [illegible] 4	589 99	10 98	68 53 28 13	640 75	7 59	30
40	9 48 8 52	553 41	3 79	38 14 [illegible]	590 24	10 99	69 4 8 89	640 95	7 54	20
50	9 57 21 96	553 52	3 85	38 24 [illegible]	590 [illegible]	11 01	69 14 49 84	641 17	7 49	10
11 0	10 6 35 48		3 91	38 34 7 2		11 02	69 25 31 01		7 44	19 0
		553 62			590 84			641 40		
10	10 15 49 10	553 72	3 96	38 43 58 07	591 12	11 04	69 36 12 41	641 61	7 39	50
20	10 25 2 82	553 82	4 02	38 53 49 1	591 41	11 05	69 46 54 02	641 81	7 33	40
30	10 34 16 61	553 9	4 08	39 3 40 [illegible]	591 7	11 06	69 57 35 83	642 02	7 28	30
40	10 43 30 50	554 [illegible]	4 1	39 13 32 [illegible]	59 [illegible]	11 0	70 8 17 8	642 23	7 23	20
50	10 52 44 58	554 12	4 19	39 23 24 3	[illegible]	11 09	70 19 0 6	642 42	7 17	10
12 0	11 1 58 70		4 21	39 33 16		11 10	70 29 42 50		7 12	18 0
		554 25			592 [illegible]			642 63		
10	11 11 12 95	554 [illegible]	[illegible]	[illegible]	592 [illegible]	11 11	70 40 25 13	642 84	7 06	50
20	11 20 27 [illegible]	554 1	4 [illegible]	[illegible] 53 [illegible]	593 10	11 12	70 51 7 97	643 04	7 01	40
30	11 29 41 71	554 6	4 41	40 [illegible]	[illegible]	11 14	71 1 51 01	643 25	6 95	30
40	11 38 56 27	554 [illegible]	4 47	40 12 4 [illegible]	593 76	11 1	71 12 34 26	643 43	6 89	20
50	11 48 10 93	554 78	4	[illegible] 2 [illegible]	594 07	11 16	71 23 17 69	643 63	6 84	10
13 0	11 57 25 71		4 [illegible]	[illegible]		11 17	71 34 1 32		6 78	17 0
		554 [illegible]			[illegible] 34			643 81		
10	12 6 40 60	554 [illegible]	[illegible]	40 2 [illegible]	594 63	11 18	71 44 45 13	644 01	6 72	50
20	12 15 55 50	555 [illegible]	[illegible]	[illegible] 2 [illegible]	594 90	11 19	71 55 29 14	644 23	6 66	40
30	12 25 10 71	555 [illegible]	4 75	41 2 [illegible] 47	595 23	11 20	72 6 13 37	644 38	6 61	30
40	12 34 25 9	555 [illegible]	[illegible]	41 12 1 [illegible]	595 [illegible]	11 21	72 16 57 75	644 59	6 55	20
50	12 43 41 30	555 [illegible] 9	[illegible]	41 22 11 [illegible]	595 80	11 21	72 27 42 34	644 77	6 50	10
14 0	12 5 50 79		4 91	41 [illegible] 7 [illegible]		11 22	72 38 27 11		6 44	16 0
		555 9			[illegible] 13			644 96		
10	13 2 12 [illegible]	555 [illegible]	4 97	41 42 3 19	596 43	11 23	72 49 12 07	645 13	6 38	50
20	13 11 28 09	555 [illegible]	5 02	41 51 [illegible] 2	596 75	11 24	72 59 57 20	645 31	6 32	40
30	13 20 43 94	555 [illegible]	5 [illegible]	42 1 [illegible]	596 98	11 21	73 10 42 51	645 50	6 26	30
40	13 [illegible] 99	556 00	5 13	42 11 [illegible]	597 31	11 25	73 21 28 01	645 68	6 21	20
50	13 39 1 [illegible]	556 [illegible] 3	5 19	42 21 0 70	597 60	11 26	73 32 13 69	645 84	6 15	10
15 0	13 [illegible] 32 22		5 24	42 31 48 30		11 26	73 42 59 53		6 09	15 0
		556 25			597 93			646 05		
10	13 57 4 57	556 [illegible]	5 [illegible]	42 41 40 23	598 24	11 27	73 53 45 58	646 20	6 03	50
20	14 7 5 06	556 [illegible]	5 35	42 51 44 47	598 53	11 27	74 4 31 78	646 36	5 97	40
30	14 16 21 67	556 74	5 40	43 1 45 00	598 79	11 28	74 15 18 14	646 55	5 91	30
40	14 25 38 41	556 80	5 46	43 11 41 79	599 14	11 28	74 26 4 69	646 71	5 84	20
50	14 34 [illegible] 30	557 01	5 51	43 21 40 93	599 40	11 29	74 36 51 40	646 88	5 78	10
16 0	14 44 12 31		5 56	43 31 40 33		11 29	74 47 38 28		5 72	14 0
		[illegible]			599 75			647 04		
	Signs V and XI			Signs IV and X			Signs III and IX			

TABLE continued.

Argu-ment ☾'s Long.	Signs 0 and VI R A	Diff	Sec. Var	Signs I and VII R A	Diff	Sec. Var	Signs II and VIII R A	Diff	Sec. Var	Argu-ment ☾'s Long.
° ′	° ′ ″ ‴	″ ‴		° ′ ″ ‴	″ ‴		° ′ ″ ‴	″ ‴		° ′
10	14 53 29 47	557 30	[illegible]	43 41 [illegible]	[illegible]	11 2[illegible]	74 58 2[illegible]	647 21	5 65	50
20	15 2 46 77	557 45	[illegible]	43 51 [illegible]	[illegible]	11 [illegible]	75 9 12 [illegible]	647 37	5 60	40
30	15 12 4 27	557 58	[illegible]	44 1 10 [illegible]	[illegible]	11 [illegible]	75 19 [illegible]	647 51	5 54	30
40	15 21 21 80	557 73	[illegible]	44 11 41 [illegible]	[illegible]	11 30	75 30 47 41	647 [illegible]	6 17	20
50	15 30 39 53	557 86	5 82	44 21 1[illegible]	[illegible]	11 [illegible]	75 41 [illegible]	647 81	5 41	10
17 0	15 39 57 [illegible]		5 8[illegible]	44 31 13 21	[illegible]	11 [illegible]	75 52 [illegible] 91		5 35	31 0
		558 02			[illegible]			647 98		
10	15 49 15 11	558 15	[illegible]	44 [illegible]	[illegible]	11 30	76 3 10 92	648 16	5 29	50
20	15 58 [illegible]	558 31	[illegible]	44 [illegible]	[illegible]	11 [illegible]	76 13 59 05	648 30	5 22	40
30	16 7 51 [illegible]	558 48	[illegible]	45 [illegible]	[illegible]	11 30	76 24 47 [illegible]	648 44	5 16	30
40	16 17 10 [illegible]	558 60	6 0[illegible]	45 [illegible]	[illegible]	11 30	76 35 35 82	648 59	5 10	20
50	16 26 [illegible]	558 77	6 1[illegible]	45 [illegible]	[illegible]	11 30	76 46 24 41	648 75	5 [illegible]	10
18 0	16 3[illegible] [illegible] 72		[illegible]	[illegible]	60[illegible]	11 30	76 57 13 16		4 97	12 0
		558 91			60[illegible]			648 57		
10	16 45 6 [illegible]	559 07	[illegible]	45 42 0 [illegible]	[illegible]	11 30	77 8 20 [illegible]	649 03	4 91	50
20	16 54 25 70	559 2[illegible]	[illegible]	[illegible]	6 [illegible]	11 30	77 18 51 06	649 16	4 84	40
30	17 [illegible] 44 [illegible]	559 [illegible]	6 [illegible]	[illegible] 2 [illegible]	[illegible]	11 30	77 29 40 22	649 30	4 78	30
40	17 13 4 [illegible]	559 [illegible]	[illegible]	46 1[illegible] 12 1[illegible]	[illegible]	11 29	77 40 29 52	649 44	4 71	20
50	17 [illegible] 23 96	559 72	6 [illegible]	[illegible] 2 1[illegible]	[illegible]	11 29	77 51 18 96	649 57	4 65	10
19 0	17 31 4[illegible] [illegible]		6 [illegible]	[illegible] 2 21 [illegible]	[illegible]	11 28	78 2 8 [illegible]		4 [illegible]	11 0
		559 86			[illegible] 19			649 70		
10	17 41 3 [illegible]	560 03	[illegible]	[illegible] 1[illegible] 20 [illegible]	[illegible]	11 28	78 12 58 2[illegible]	649 83	4 52	50
20	17 50 [illegible]	560 20	[illegible]	[illegible] 2 [illegible]	6[illegible] 5 61	11 28	78 23 48 06	649 [illegible]	4 45	40
30	17 59 13 07	560 36	6 64	47 [illegible]	[illegible] 07	11 27	78 34 38 03	650 10	4 39	30
40	18 9 4 [illegible]	560 52	[illegible]	47 [illegible] 14	[illegible] 6 41	11 27	78 45 28 1[illegible]	650 21	4 32	20
50	18 18 24 [illegible]	560 71	[illegible]	47 [illegible]	[illegible] 69	11 26	78 56 18 34	650 33	4 25	10
20 0	18 27 45 26		[illegible]	47 [illegible] 2 7 2[illegible]		11 26	79 7 8 6[illegible]		4 19	10 0
		560 87			[illegible]			650 46		
10	18 37 6 13	561 03	6 84	47 [illegible]	607 32	11 25	79 17 59 13	650 58	4 12	50
20	18 46 27 16	561 20	6 9[illegible]	47 [illegible]	607 57	11 24	79 28 49 71	650 70	4 06	40
30	18 55 48 36	561 39	6 91	48 3 19 12	607 03	11 24	79 39 40 41	650 81	3 99	30
40	19 5 9 75	561 55	6 98	48 13 27 05	60[illegible] 21	11 23	79 50 31 22	650 93	3 92	20
50	19 14 31 30	561 73	7 03	48 23 35 2[illegible]	60[illegible]	11 22	80 1 22 15	651 03	3 86	10
21 0	19 23 53 03		7 08	48 33 [illegible] 77		11 21	80 12 13 18		3 79	9 0
		561 91			[illegible] 3			651 15		
10	19 33 14 94	562 09	7 13	48 43 [illegible]	609 11	11 21	80 23 4 33	651 25	3 72	50
20	19 42 37 03	562 26	7 18	48 54 1 71	609 43	11 20	80 33 55 58	651 37	3 65	40
30	19 51 59 29	562 45	7 22	49 4 11 1[illegible]	609 72	11 19	80 44 46 9[illegible]	651 46	3 59	30
40	20 1 21 74	562 61	7 27	49 14 20 86	610 07	11 18	80 55 38 41	651 57	3 52	20
50	20 10 41 35	562 82	7 32	49 24 30 93	610 33	11 17	81 6 29 98	651 67	3 45	10
22 0	20 20 7 17		7 37	49 34 41 26		11 16	81 17 21 6[illegible]		3 38	8 0
		563 00			610 62			651 76		
10	20 29 30 17	563 19	7 4[illegible]	49 44 [illegible]	610 96	11 15	81 28 13 11	651 86	3 32	50
20	20 38 53 36	563 36	7 46	49 5[illegible] 2 84	611 23	11 14	81 39 5 27	651 96	3 25	40
30	20 48 16 72	563 57	7 51	50 5 14 07	611 55	11 13	81 49 57 23	652 04	3 18	30
40	20 57 40 29	563 74	7 55	50 15 25 62	611 85	11 11	82 0 49 27	652 14	3 11	20
50	21 7 4 03	563 92	7 60	50 25 37 47	612 16	11 10	82 11 41 41	652 23	3 04	10
23 0	21 16 27 95		7 65	50 35 49 63		11 09	82 22 33 64		2 97	7 0
		564 13			612 44			652 31		
10	21 25 52 08	564 35	7 69	50 46 2 07	612 77	11 08	82 33 25 95	652 40	2 91	50
20	21 35 16 13	564 50	7 74	50 56 14 84	613 07	11 06	82 44 18 3[illegible]	652 48	2 84	40
30	21 44 40 93	564 71	7 78	51 6 27 91	613 37	11 05	82 55 10 83	652 56	2 77	30
40	21 54 5 61	564 92	7 82	51 16 41 28	613 65	11 03	83 6 3 39	652 64	2 70	20
50	22 3 30 56	565 09	7 87	51 26 54 9[illegible]	613 95	11 02	83 16 56 03	652 70	2 63	10
24 0	22 12 55 65		7 91	51 37 8 88		11 00	83 27 48 73		2 56	0
		565 32			614 26			652 79		
	Signs V and XI			Signs IV and X			Signs III and IX			

TABLE continued.

Argu-ment. ☉'s Long.	Signs 0 and VI. R. A.	Diff.	Sec. Var.	Signs I. and VII. R. A.	Diff.	Sec. Var.	Signs II. and VIII. R. A.	Diff.	Sec. Var.	Argu-ment. ☉'s Long.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
10	22 22 20·97	565·49	7·96	51 47 23·14	614·56	10·99	83 38 41·52	652·87	2·49	50
20	22 31 46·46	565·73	8·00	51 57 37·70	614·88	10·97	83 49 34·39	652·93	2·42	40
30	22 41 12·19	565·88	8·04	52 7 52·58	615·16	10·95	84 0 27·32	653·00	2·35	30
40	22 50 38·07	566·13	8·09	52 18 7·74	615·46	10·94	84 11 20·32	653·06	2·28	20
50	23 0 4·20	566·30	8·13	52 28 23·20	615·75	10·92	84 22 13·38	653·13	2·21	10
25 0	23 9 30·50	566·53	8·18	52 38 38·95	616·07	10·90	84 33 6·51	653·19	2·14	5 0
10	23 18 57·03	566·73	8·22	52 48 55·02	616·37	10·88	84 43 59·70	653·25	2·07	50
20	23 28 23·76	566·94	8·26	52 59 11·39	616·66	10·86	84 54 52·95	653·31	2·00	40
30	23 37 50·70	567·14	8·30	53 9 28·05	616·93	10·85	85 5 46·26	653·37	1·93	30
40	23 47 17·84	567·37	8·35	53 19 44·98	617·27	10·83	85 16 39·63	653·41	1·86	20
50	23 56 45·21	567·57	8·39	53 30 2·25	617·54	10·81	85 27 33·04	653·48	1·79	10
26 0	24 6 12·78	567·79	8·43	53 40 19·79	617·83	10·79	85 38 26·52	653·51	1·72	4 0
10	24 15 40·57	567·98	8·47	53 50 37·62	618·15	10·77	85 49 20·03	653·57	1·65	50
20	24 25 8·55	568·23	8·51	54 0 55·77	618·43	10·75	86 0 13·60	653·62	1·57	40
30	24 34 36·78	568·42	8·55	54 11 14·20	618·73	10·73	86 11 7·22	653·65	1·50	30
40	24 44 5·20	568·65	8·59	54 21 32·93	619·05	10·70	86 22 0·87	653·70	1·43	20
50	24 53 33·85	568·86	8·63	54 31 51·98	619·31	10·68	86 32 54·57	653·74	1·36	10
27 0	25 3 2·71	569·09	8·67	54 42 11·29	619·62	10·66	86 43 48·31	653·78	1·29	3 0
10	25 12 31·80	569·29	8·71	54 52 30·91	619·89	10·64	86 54 42·09	653·81	1·22	50
20	25 22 1·09	569·54	8·75	55 2 50·80	620·22	10·62	87 5 35·90	653·84	1·15	40
30	25 31 30·63	569·74	8·79	55 13 11·02	620·49	10·59	87 16 29·74	653·88	1·08	30
40	25 41 0·37	569·98	8·83	55 23 31·51	620·78	10·57	87 27 23·62	653·91	1·01	20
50	25 50 30·35	570·20	8·87	55 33 52·29	621·09	10·54	87 38 17·53	653·93	0·93	10
28 0	26 0 0·55	570·41	8·91	55 44 13·38	621·35	10·52	87 49 11·46	653·95	0·86	2 0
10	26 9 30·96	570·64	8·95	55 54 34·73	621·67	10·49	88 0 5·41	653·99	0·79	50
20	26 19 1·60	570·89	8·99	56 4 56·40	621·93	10·47	88 10 59·40	654·00	0·72	40
30	26 28 32·49	571·11	9·03	56 15 18·33	622·24	10·44	88 21 53·40	654·02	0·65	30
40	26 38 3·00	571·34	9·06	56 25 40·57	622·52	10·42	88 32 47·42	654·04	0·58	20
50	26 47 34·94	571·58	9·10	56 36 3·09	622·83	10·39	88 43 41·46	654·04	0·50	10
29 0	26 57 6·52	571·79	9·14	56 46 25·92	623·08	10·36	88 54 35·50	654·07	0·43	1 0
10	27 6 38·31	572·02	9·17	56 56 49·00	623·39	10·34	89 5 29·57	654·07	0·36	50
20	27 16 10·33	572·28	9·21	57 7 12·39	623·67	10·31	89 16 23·64	654·08	0·29	40
30	27 25 42·61	572·51	9·25	57 17 36·06	623·94	10·28	89 27 17·72	654·09	0·22	30
40	27 35 15·12	572·74	9·28	57 28 0·00	624·26	10·25	89 38 11·81	654·10	0·14	20
50	27 44 47·86	572·98	9·32	57 38 24·26	624·50	10·22	89 49 5·91	654·09	0·07	10
30 0	27 54 20·84		9·35	57 48 48·76		10·19	90 0 0·00		0·00	0 0
	Signs V. and XI.			Signs IV. and X.			Signs III. and IX.			

N. B. If the ☉'s longitude is between 0 and III. signs, the Table gives the R. A. If between III. and VI. signs, subtract the R. A. given by the Table from 180°, the remainder is the R. A. If between VI. and IX. signs, add 180° to the R. A. as taken from the Table. But if between IX. and XII. signs, subtract the result given by the Table from 360°. The secular equation of the ☉'s R. A. is additive if subsequent to 1801, otherwise subtractive from the R. A. as given by the Table from the above directions.

V. *Answer*

V. *Answer of Mr.* P. Nicholson *to Mr.* Holdred *on Mr.* N.'s *Work on Involution and Evolution.*

To Mr. Tilloch.

Sir, — Permit me to offer a few observations in my own defence in reply to what appear to me the very illiberal and invidious insinuations of your correspondent, Mr. Holdred, in the Philosophical Magazine for November 1820, upon my recent work on Involution and Evolution, published about the middle of April 1820, and

You will oblige your most obedient servant,

Gower-Place, Euston-Square, Jan. 12, 1821. P. Nicholson.

It is not my intention to review Mr. Holdred's work, or to answer his calumnies, in any other way than by contradicting some of his principal assertions, which might otherwise have a tendency to prejudice the reader, who might not have had an opportunity of examining the Essay which I have published, and his recent tract on the Resolution of Equations; and by placing the points in dispute, in such a light that the candid and ingenuous reader may be able to judge for himself of the merits of the case. I shall therefore confine myself to the two following points:

1st. He says the article which I put into the Philosophical Magazine for October 1818, was taken from his method of extracting the cube root, and that this has been the means by which Mr. Horner has become acquainted with the principle.

2d. He says also that the Essay on Involution and Evolution published by me about the middle of April 1820, was, exclusively, his and Mr. Horner's. I am under no necessity to prove it is not Mr. Horner's, but I can make it appear sufficiently evident that the nonfigurate method first published by the above gentleman did not originate with Mr. Holdred.

The following quotation from your correspondent will evidently show his unfair disposition towards me, excited by my having published my own improvements of his method of extracting the roots of equation, the principle of which he had at first but very rudely suggested.

"After Mr. Nicholson had discovered another manner of demonstration, he requested me to annex it to my tract by way of Supplement, *lest any one should discover the same way of demonstrating the rule after it should be published as quickly as he had done before.*"

I shall here observe, that the words put in italics are fabri-

cated entirely by Mr. Holdred: but it is possible that if intended to convey any unpleasant reflection upon me, they perhaps with more justice might be applied to himself. In a qualified sense they apply to all writers; and hence arises the idea of the patent and copy-right which the wisdom of the legislature has granted to meritorious individuals who have benefited mankind by their discoveries—thus protecting such persons in the advancement of their reputation, and in the procuring of emolument; it is therefore no stigma upon a man to wish to enjoy the fruit of his own labour.

With respect to the article which I put into the Philosophical Magazine 1818, it was a general demonstration of the polynomial $(a + b + c + d + \&c.)^n$ with its application to Involution and Evolution; this article was shown to Mr. Holdred before it was committed to the press.

I spoke in handsome terms of the work which he was about to publish; and this was the only time that ever I received his thanks, notwithstanding the instructions I gave him in maturing his work.

I never in my life borrowed a single idea from Mr. Holdred, or any other man living, without acknowledging it in writing as theirs: but this is not the case with Mr. H. I only desire the reader who may be interested in this branch of analysis to compare the article I have just referred to with what Mr. H. has since produced, and say whether or not Mr. Horner could have derived any advantage from it. It is just as possible that he might have made the discovery by hearing that some one had done it; but it appears to me much more probable he took the germ of his idea from Budan, to whose method it has a much nearer affinity. It is not uncommon, when a discovery is made by any particular person, that more than one will lay claim to it. Mr. Horner, by report, is a gentleman, and from what I have seen he appears to be an excellent mathematician, and I have not the smallest doubt of his inventive powers.

It is painful to me to be under the necessity of exposing Mr. Holdred by entering more minutely into his work; but this must be done in my own defence, in order that the reader may understand the difference of our methods of demonstrating the rule.

The account which Mr. Holdred has given in his preface, of the origin of the rule which he was not able to demonstrate without assistance, is not that which he stated to me. When I put the question to him, he answered, he had taken his ideas from an example in Ward's Mathematics, which he pointed out

to

to me, and is to be found in chap. x. sec. 2. where the same rule is applied to a quadratic equation*.

This idea only wanted generalizing.

He was acquainted with the numeral exegesis to which his method bears a striking affinity, but he was an entire stranger to what had been done by either Newton or Ralphson, before I pointed out their methods to him.

If he had known Sir Isaac's method of transforming equations, which is that from which I derived my ideas, and not from his crude notions, he would have been ashamed to say that "Mr. Nicholson extracted all from me, even his demonstrations are mine." (Vide The Philosophical Magazine, Nov. 1820.) He certainly must entertain a very mean opinion of such as might be interested at all in the discovering and maturing of the subject, to suppose that they would take either his *ipse dixit* or mine, without convincing themselves by a diligent perusal and investigation of the method, and an attentive perusal of the Essay which I published on Involution and Evolution, with the Postscript.

With respect to what he calls his demonstration, the first step is the same with what has been done both by Newton and Ralphson. Suppose, for instance, that the equation is in x, he takes r for the first figure of the root and y for the remaining part of it, and substitutes $r+y$ for x, and thus the original equation is transformed to another in y; he then takes a for the second figure of the root, and u for the remaining part of it; so that $x=r+a+u$, and to reduce this to the binomial form, he puts ${}_2r=r+a$ and substitutes ${}_2r+u$ for x. By this means he gets an equation in u where the coefficients are expressed by the coefficients of the original equation and ${}_2r$, and therefore reinstates $r+a$ for ${}_2r$ in all the coefficients of the powers of u, and thus forms as many subsidiary equations as there are units in the exponent of the highest power: therefore if n be the exponent of the power, the number of equations that each digitical figure of the root will require after the first will be $n+1$.

In these equations the coefficients do not exhibit the figurates which it is his object to elicit; consequently, he has recourse to a most tedious and circumlocutory explanation in words. This demonstration, as Mr. Holdred calls it, is extended to eight quarto pages.

In page 5 of this work he asserts, that "B + C + D + &c. is *an imperfect divisor* by which the next figure of the root may be discovered, which when found I call *a*." Similar assertions will be found in pages 12, 15, 17, 18, 20, 25 and 26. This is equivalent to saying, that the sum of the coefficients of the powers of

* I have seen since the same rule applied to a quadratic equation in Emerson's Algebra.

all

an unknown quantity in a transformed equation, will be the proper divisor for ascertaining the next figure of the root; but certainly nothing can be more absurd or destitute of principle, and this one instance is sufficient to show that he never had any clear ideas of treating the subject.

The manner in which I first formed my ideas on the extraction of the roots of equations, will be found in the Introduction to the Essay on Involution and Evolution. I shall therefore not take up the reader's time by repeating it here, but proceed to give an example of my method of demonstrating the rule, as I have now just done by the method of Mr. Holdred, which the candid reader will no doubt feel to be the most eligible and satisfactory method of settling the dispute.

I take a, b, c, &c. for each part of the root found by the corresponding step. In the first equation I substitute $a+u$ for x, and obtain an equation in u; but in the next equation, instead of u, I substitute $b+v$, and obtain a new equation in v and so on. So that in the different steps the equations which I produce are the same as Sir Isaac Newton's, except that the orders of figurate numbers are clearly exhibited; and thus to prove every portion of the root, only one equation is required. But by the method of Mr. Holdred, every portion of the root after the first would require as many equations, and one more, as the exponent of the highest power has units. As my method is founded on the principle of Sir Isaac, I have, from its simplicity, been able to comprise the demonstration of the figurate method within the limits of two octavo pages.

With regard to the non-figurate method, the fact is, that after the rule has been demonstrated by my method, or any other, no other demonstration is necessary to perform the operation, either by Mr. Horner's method, by Mr. Holdred's, or my own, as all the different forms arise from the manner of summing up the quantities; viz. by writing the numbers and their sums under them, or writing down the sums as each number is added, with the number to be added, or writing the first number and the successive sums only. As, for example, let m and n be any two consecutive coefficients of the original equation, and let p, q, r, &c. be any numbers to be added to m, and let $\mathrm{N}=m+p$. These different forms of addition are explained by the following operations:—

No. 1.

$$\begin{array}{r|l}
m & n \\
p & \mathrm{N}a \\
q & \mathrm{N}a+\ qa \\
r & \mathrm{N}a+\ qa+ra \\
\hline
\mathrm{N}+q+r & 3\mathrm{N}a+2qa+ra+n=\mathrm{Q}
\end{array}$$

Here every number on the right-hand of the line, after the second, is found by multiplying the opposite number on the left

left by a, and adding the product to the number above that which is to be found; and the quantities below the horizontal line, are the sum of those above it: the same is to be understood in the following modes:—

No. 2.

$$
\begin{array}{r|lcl}
m & n & = & Q \\
p & Na & = & R \\
\hline
N & Na + \quad n & = & {}_{1}Q = Q + R \\
q & Na + \quad qa & = & S \\
r & Na + \quad qa + ra & = & T \\
\hline
N + q + r & 3Na + 2qa + ra + n & = & {}_{3}Q = Q + R + S + T
\end{array}
$$

No. 3. — or thus, according to my notation.

$$
\begin{array}{lr|lcl|l}
P = & & & = & Q & Q \\
{}_{1}P = & & Na + \quad n & = & {}_{1}Q & {}_{1}P\,{}_{1}Q = Q + {}_{1}Pa \\
{}_{2}P = & N + q & 2Na + \quad qa + n & = & {}_{2}Q & {}_{2}P\,{}_{2}Q = {}_{1}Q + {}_{2}Pa \\
{}_{3}P = & N + q + r & 3Na + 2qa + ra + n & = & {}_{3}Q & {}_{3}P\,{}_{3}Q = {}_{2}Q + {}_{3}Pa
\end{array}
$$

It is evident that ${}_{3}Q$ will be found the same, whichever of these methods of adding the numbers together is adopted; or since the producing numbers in the right-hand column are Q, R, S, T, all the methods of addition will be shortly represented as follows:

First method.	Second method.	Third method.
Q	$Q = Q$	$Q = Q$
R	${}_{1}Q = Q + R$	R
S	${}_{2}Q = Q + R + S$	${}_{1}Q = Q + R$
T	${}_{3}Q = O + R + S + T,$	S
${}_{3}Q = Q + R + S + T,$		${}_{2}Q = Q + R + S$
		T
		${}_{3}Q = Q + R + S + T$

The first method is the form of addition adopted in my Essay on Involution and Evolution; the second, is that adopted in the Postscript; and the third is that adopted by Mr. Holdred, which he calls an invention; but since he ascribes to himself the merit of having employed this peculiar mode of addition, it is but justice to myself to apprize the reader that he has taken the hint from what is everywhere done in my book on Involution and Evolution, as may be seen in the first two lines of every operation from page 42 to page 48 inclusive. The method of proof which he pretends to have discovered (page 47 of his Supplement) is also the very same in principle as is found in that work of mine in the two lines referred to. I should not have noticed these things, which to

some

some may appear trifles, and which consist in mere modes of addition, had he not noticed them himself. My object was, to divest the operation of all numbers which were not expressed by my formula, and I have succeeded so far as to do it. I cannot help observing, that nothing but the desire of being thought the author of a useful discovery, ignorance of what has been before published in the writings of men of superior intellect, and an overweening opinion of what he himself has understood but very imperfectly, could make him so roundly assert that I learned all from him; while, on the contrary, he availed himself of no hint of mine. At the time I published the work now mentioned, I was divided in my own opinion which of the methods I should adopt; but as I had not then wrought any example, it occurred to me that the form which I had adopted was that which produced the fewest numbers; but I did not perceive, at that time, that it would occasion more lines: however, I saw afterwards the advantage of adopting the method used in the postscript, not only in saving lines, but that it agreed exactly with the simplicity of the formula which I had already investigated and adopted, and that it was much better adapted to the transformations of equations, particularly in diminishing the root by unity at each step. These were the motives that induced me to make the change in adding the numbers together; and that this was really the case, I have credible witnesses to confirm what I here assert. Had I adopted Mr. Holdred's clumsy method of adding every two lines together, I should not have been able to bring it within a moderate compass, and should, besides, have obscured the principle. In my arithmetical process, according to the figurate method, the numbers stand exactly in the same manner as indicated in the coefficients of my transformed equations; and in my process according to the non-figurate method, the different steps of the operation are performed by a general formula, which is the result of a property of figurate numbers, and which gives the very same orders as are expressed in the coefficients of every general transformed equation. On account of the course of conduct pursued by Mr. Holdred, the non-figurate method would have been published immediately after the rudiments of algebra, in my last work entitled Analytical and Arithmetical Essays, published near the end of Nov. 1820, though dated 1821, whether Mr. Horner's method of continuous approximation had appeared or not, as I can prove if it were necessary; but understanding that Mr. Holdred's book was in the press, I laid the work above alluded to aside, and published my demonstrations and methods by themselves, in the Essay on Involution and Evolution, before Mr. Holdred's work made its appearance. Let any one compare the simple but general result in page 37 of my Essay, with his unsatisfactory

tisfactory method, which only shows the truth of the rule as applied to the equation he has proposed. He has not given such a demonstration as to furnish a general rule for all equations; but, reasoning by analogy, he has presumed the truth of the rule without demonstration, by extending the idea to a general inference. This evident impropriety proceeds from his total ignorance of the properties of figurate numbers, on which the whole depends; but on this head, he ungenerously observes, he did not want any of my assistance.

From what I have substantiated here, I shall be warranted in restating what I have asserted in page 57 of the postscript, viz. that Mr. Holdred's book does not contain a single idea but what is found in my Essay on Involution &c., and which was given to the world prior to the publication of his tract.

In the postscript, page 64, which was written in consequence of the misrepresentations to be found in Mr. H.'s preface, I have stated the circumstances which led me to the demonstration of the non-figurate method, and have there given a comparative view of our methods by actual examples, from which the inquiring reader may see, that if I was not the original inventor of the principle, I have improved the demonstration and have simplified the practice of both methods; but with respect to the non-figurate operation, Mr. Holdred has no claim to style himself the original inventor of it, as I was the first to give him any hint of it by signifying my intention to him (in confidence) of reducing it to a formula, similar to that which I published in Essay 3, page 4, of my Combinatorial Essays. However, I cannot help remarking, that if what I have done is not purloined from him, it certainly has flowed from what he has done, as a consequence; for it was undoubtedly in consequence of the conduct of Mr. Holdred that I was induced to publish, in my own defence, my own improvements in extracting the roots of equations; for I was, prior to our difference, resolutely bent on relinquishing the study of the mathematics, which interrupted the progress of my professional duties, and of other publications which I was then, and am still, engaged in; being fully aware, from dear-bought experience, that analytical researches not only occupied too much of my time, but obliged me to expend that money which I could have appropriated in a much more eligible and advantageous way.

If, after all that has been not only said, but proved, Mr. Holdred should still persist in asserting that my demonstrations were extracted from his, I call upon him to show the reason why he has occupied twelve quarto pages in the demonstration of both methods, when all that he has said might be comprised within the compass of one-twelfth part of the space, which is what I have done. It appears then, that either my demonstrations must

be

be deficient, or his redundant; and I think, and the reader will most assuredly think also, that Mr. Holdred, after the bold assertions which he has made, is imperiously called upon to demonstrate which of the two is the case. Though Mr. Holdred's work has been the result of forty years experience and consideration, he has not applied his rules to those cases in the extraction of roots where there is any real difficulty.

Such as wish to see the rules for extracting the roots of equations, derived by me from Sir Isaac Newton's method of transforming equations, may consult my Analytical and Arithmetical Essays, where the transformation of equations, and the determinations of the limits of their roots, I hope are fully considered.

VI. *On the Electro-Magnetic Experiments of MM.* Œrsted *and* Ampere. *By Mr.* Hatchett.

The use of the compass in France takes date from the year 1260. The principal part of this instrument, as the reader is aware, consists in a magnetized steel needle, of the form of a very elongated lozenge. This needle, moveable round a vertical axis, brings itself on every spot of the earth to an equilibrium in a vertical plane, which is named *the magnetic meridian.* The angle which this plane makes, with that of the astronomic meridian of the place where the observation is made, is called the *declination of the compass.* In 1580, this declination was at Paris 11° 30′ towards the west; in 1663, nothing; and in 1819 (22d April), 22° 19′ west. If the declination of a magnetised needle changed neither with time nor place, or at least if the changes were made according to known laws, the science of navigation would possess an instrument of simple construction, easy to observe with, and precious indeed to mariners, who would find in a needle, the intrinsic value of which is almost nothing, the only means of steering their course when night and clouds veil the sky. Philosophers at first endeavoured, but in vain, to discover the cause of the phænomena which the magnetised needle presents. They, nevertheless, succeeded in giving to a bar of steel that singular property of the natural magnet, of taking at each place of the earth a position, the diurnal or secular variations of which are periodical. They have studied and measured with care the magnetic attractions and repulsions. The labours of Coulomb, the instruments invented by that celebrated philosopher, and those which M. Lenoir, the distinguished artist of the Bureau of Longitude, has executed, have considerably improved the science of magnetism. M. Œrsted, Professor of the University of Copenhagen, has just opened a new field to the inquiries of philosophers. It is

to

to him that we owe that fine observation, that *a metallic wire, which communicates with the two extremities of a Voltaic electrical apparatus, acquires the very remarkable property of acting at a distance on a magnetic needle.* This metallic wire has been named the *conjunctive wire.*

It was already known, that by augmenting the surfaces of the metallic plates which compose the electrical apparatus of Volta, and uniting the two wires which communicate with the extreme plates of that apparatus, these wires become heated, redden, and burn in atmospheric air. M. Thenard and myself had made that experiment in 1801. (See No. 11 of the Journal of the Polytechnic School, p. 291.) The conjunctive wire, in the experiment of M. Œrsted, will become heated, but if it is of sufficient diameter it will not burn; and its action may be observed on a magnetic needle at some distance.

For twenty-three years the electric piles of Volta had been in use, and no philosopher had yet thought of bringing a magnetic needle near one of these piles in action. This inspiration was reserved to M. Œrsted; and it must be confessed, that chance had much less share in it than in many discoveries with which physical science has been enriched.

M. Marcel de Serres translated from the German, and published in 1807, a work of the Danish Professor entitled *An Inquiry into the Identity of Chemical and Electrical Forces.* It may be seen from chapter 8 of that work, that the author had been led by his subject to seek proofs of the identity of the magnetic and electric forces*. He had proposed to try *whether electricity the most*

* There is nothing to be found in this chapter which establishes, in any manner, the identity of magnetism and electricity. It is even remarkable, that when M. Œrsted had discovered the action of the conjunctive wire of the Voltaic pile upon the magnetic needle, he explained this new phænomenon by a hypothesis which supposes that the negtive electricity acts only on the northern pole of the needle, and positive electricity on the southern: (see *Annales de Chimie* for Aug. 1820, p. 244.)—a fact, which would establish a total difference between the electric and magnetic fluids, since the magnetic fluid, whether considered as positive or negative, ought to act equally on both poles. To demonstrate, by experiment, the identity of the electric and magnetic fluids, it was necessary to show that that could explain all the phænomena which could be observed, whether in the mutual action of two magnets, or in the action of a conjunctive wire upon a magnetic needle, without admitting in a magnet any other fluid than the acting electric fluid, as in bodies which are not susceptible of magnetism; and to tell how this electric fluid is disposed in the magnet. It is this which M. Ampere has done, by demonstrating 1st, That two conjunctive wires, of metals not magnetic, attract and repel through the intervention of the electric fluid alone; 2d, that a magnet may be substituted for one of the conjunctive wires, without any change in the nature of the action taking place; 3d, that the second conjunctive wire may be removed for another magnet,

most latent, has any action on the magnet. Now, the electricity in the conjunctive wire of an electrical apparatus in action, is indeed latent, since it does not manifest itself to any electrometer; and, in fact, M. Œrsted performed last winter (1819) the experiment which justified what he had conceived seven years before. The result of this experiment has been known in Paris only three months, and already several distinguished philosophers have deduced from it most important consequences, both for magnetism and electricity. We shall give an account of these as succinctly as possible.

Supposing the metallic plates which form the electrical apparatus with troughs, to begin with zinc and finish with copper, the electrical current, supposed to be in the conjunctive wire, would go from the first plate to the last. Now, imagine another conjunctive wire of the same apparatus, placed parallel to the first, and disposed in such a manner that it may transmit an electrical current in a direction contrary to the first, the two wires will repel. If the currents are in the same direction, they will attract. M. Ampere was the first to observe these attractions and repulsions at a distance, between bodies traversed by an electric fluid which does not manifest any tension.

M. Arago magnetised a slip of iron, and afterwards a steel wire, by putting them in contact with, or under the influence of, the conjunctive wire. A simple method of magnetising a steel needle by the conjunctive wire, consists in placing the needle in the part of the conjunctive wire which is twisted spirally: whether the needle is placed directly upon the threads of the spiral, or enveloped in paper or a glass tube to prevent contact with the conjunctive wire, it becomes magnetised, and its north and south poles, corresponding to the north and south poles of the terrestrial magnet, will be determined by the direction of the spiral which bears the needle. If the conjunctive wire be placed in a vertical plane, and in the direction of the electric current which passes from the zinc plate of the apparatus to the copper plate, the generating point of the spiral may turn from left to right or

magnet, without any other change resulting in the nature of the action, except the phænomena known to result from the mutual action of two magnets; and 4th, that the distribution of the electric fluid in the conjunctive wire, is the same as in planes perpendicular to the line which joins the two poles of a magnet, following limited curves, traced in these planes around the axis of the magnet.

M. Ampere thus established the identity of electric and magnetic fluids, while M. Arago made his fine experiment on the magnetising of slips of iron by the right conjunctive wire. Since these two philosophers have further added the magnetising of a steel bar by a conjunctive wire twisted spirally round this bar; and that they have anticipated the principal circumstances of that magnetising, it would appear, that they cannot dispense with admitting the identity of the two fluids.

the

the current, or from right to left; in the first case, the south pole of the needle, corresponding to the north pole of the terrestrial magnet, will be on the side of the zinc plate of the apparatus; in the second case, it is the north pole of the needle which is on that side.

M. Arago, following the theory of M. Ampere, conceived the idea of twisting a conjunctive wire in the manner of two symmetrical spirals placed one after the other; these spirals differed from each other only as to the direction in which their generating points turned round their hollow spindles: by putting a needle in each spindle, the two needles became magnetised at the same time, so that their poles of the same name were contiguous. In transmitting a discharge of a Leyden phial through a copper wire twisted in the same manner, in the manner of two consecutive symmetrical spirals, M. Arago has further observed, that the steel needles placed on these spirals became magnetised by the electric fluids of ordinary machines, as well as by the Voltaic apparatus.

Other facts have been long known, which prove the mutual influence of the two fluids, magnetic and electric. The points of paratonnerres become naturally magnetised by the electricity of the atmospheric air. M. Arago, author of an article on the magnetic forces, which is inserted in the Annuary of 1819, reports, as from an eye-witness, that a Genoese ship, on its way to Marseilles, was struck by the thunder at a little distance from Algiers; that the needles of the compass made all a half revolution, although these needles did not appear damaged, and the ship struck on the coast at the moment that the pilot thought he made the North Cape.

Ritter had concluded (*Journal de Physique*, t. 57. year 1803,) from some experiments, which have not been since verified, that the earth has electric poles, as it has magnetic meridians.

M. Desormes and myself had attempted in 1805 to ascertain the direction which a horizontal electric pile would take, composed of 1480 thin plates of copper, tinned with zinc, of the diameter of a five-franc piece. We placed this pile upon a boat, which floated on the water of a large vat. We knew that a magnetised steel bar, of a weight nearly equal to that of the pile, and placed like it upon the boat, would turn, after some oscillations, into the magnetic meridian. The pile, placed in the same situation, did not take any determinate direction. The only satisfaction which this pile procured us, was the recognising of the tension of the electric fluid at its extremities, without the aid of the condenser. (See the Correspondence of the Polytechnic School, tome 1. p. 151.)

M. Ampere has confirmed, by experiment, the conclusions of the Memoir which he read on the 25th September, 1820, to the

Royal Academy of Sciences, on the mutual actions of the earth, the conjunctive wires of a Voltaic apparatus, and a magnetic needle. He presented in the following sittings three new arrangements of apparatus, of his invention. The first shows a circular conjunctive wire submitted to the action of a Voltaic apparatus, and which is directed by the action of the terrestrial globe in a vertical plane, perpendicular to the plane of the magnetic meridian. The second apparatus consists in a circuit almost closed, and of a rectangular form, which turns round a horizontal axis perpendicular to the plane of the magnetic meridian, and the plane of which inclines to take, by the action of the earth, a direction perpendicular to that of the inclination of the needle. This inclination, which has not been measured with exactness for some years past, was at Paris on the 22d April 1819, 68° 25′. It is variable, like the declination, according to times and places. The third apparatus of M. Ampere exhibits a conjunctive wire twisted spirally, the extremities of which are attracted and repelled by a magnetic bar, as those of a needle would be.

The coexistence of the electric and magnetic actions has naturally led to the idea, that wires submitted to the influence of the terrestrial globe, or of a magnetic needle, may decompose water, like those which communicate with the extremities of a Voltaic pile.

The following is the account of Professor Œrsted's experiments:—

New Electro-Magnetic Experiments. By Prof. Œrsted.

Subsequently to the first experiments which I published on the magnetic action of the galvanic battery, I have extended my researches on the subject as much as various other avocations would allow me.

The intensity of the electricity seems to have no share in the magnetic effects; they depend solely on its quantity. The discharge of a strong electric battery, sent through a metallic wire, produced no change in the position of the magnetic needle. The needle is acted upon by an interrupted succession of electric sparks through the medium of the ordinary electric attractions and repulsions, but no electro-magnetic effect was produced, as far as could be perceived. In like manner a galvanic pile, consisting of 100 discs, each two inches square, and of paper moistened in salt water, to serve as a conductor of the fluid, exhibits no sensible effect upon the needle. The effect is, however, produced by a single galvanic arc of zinc and copper, with a liquid of peculiar conducting power as a conductor; for example, a liquid consisting of one part sulphuric acid, an equal part of nitric acid, and sixty parts of water. The quantity of water may even be doubled,

doubled, without the effect being greatly diminished. When the surface of the two metals is small, the effect is proportionally diminished; and *vice versa*, it is increased in proportion as the surfaces are increased. A considerable effect is obtained from a zinc plate six inches square, immersed in a copper vessed filled with the liquid conductor which I have just mentioned. An apparatus of this description, in which the surface of the zinc plate is two inches square, acts upon the needle with so much force, that the effect is sensibly felt at the distance of thee feet, and that too, when the needle is not very moveable. I have not met with any greater effects from an apparatus composed of forty similar troughs; indeed, the effect appeared somewhat diminished. I have not pursued the investigation of this point very minutely, but the observation which I have made is correct. I shall conclude that by the slight diminution of the conducting power, which results from an increase in the number of the elements of the apparatus, a diminution in the electro-chemical effect is also occasioned.

In order that the effect of a single galvanic arc may be compared with that of an apparatus consisting of several arcs or elements, the following experiment may be made. Suppose fig. 4. (Plate I.) to represent a galvanic arc, consisting of a piece of zinc *z*, of copper *e*, of a metallic wire *a b*, and of a fluid conductor *l*. The zinc invariably communicates to the water a portion of its positive electricity, and the copper a portion of its negative. In consequence of this, there would be an accumulation in the upper part of the zinc of negative electricity, and in the upper part of the copper of positive electricity, if it were not that the communication *a b* established the equilibrium, by furnishing a free passage for the negative electricity from *c* to *z*. It will be seen. then, that the wire *a b* receives the negative electricity of the zinc, and the positive electricity of the copper; while a wire, which forms the communication of the two poles of a pile, or of another compound galvanic apparatus, receives the positive electricity of the zinc pole, and the negative of the copper pole.

Paying proper attention to this distinction, all the experiments which I at first made with a compound galvanic apparatus, may be repeated with a single galvanic arc. The use of a single galvanic arc is attended with this great advantage, that it enables the experiments to be repeated with little expense and trouble. It has another advantage still more considerable, namely, that a galvanic arc may be formed of power sufficient for the electro-magnetic experiments, and yet light enough to be so suspended to a small metallic wire, as that the small apparatus may be turned round the prolonged axis of the wire. It is open, in this way, to examine

examine the action which a magnet exercises on the galvanic arc. Since no body can put another in motion, without being put in motion in its turn, when it possesses the requisite mobility, it is easy to foretel that the magnet must move the galvanic arc.

To mark the motion given by the magnet to a simple galvanic apparatus, I employed various arrangements. One of these will be found represented in fig. 5, which exhibits a perpendicular section of it in the direction of the breadth. A trough of copper *c c c c* is three inches high, four inches long, and half an inch broad; dimensions, which may of course be varied at pleasure. It may be observed, however, that the breadth should not be great, and that the plates of the trough should be as thin as possible. A plate of zinc *z z*, is kept in its position by two pieces of cork *e e; c f' f f f' z* is a brass wire, of at least a quarter of a line in diameter; *a b* is a brass wire as fine as possible, so as to be able at the same time to support the weight of the apparatus; *c a c* is a linen thread which unites the wire to the apparatus. The fluid conductor is contained in the trough. The conducting wire of this apparatus will attract the north pole of the needle when it is placed on the left side of the plane *c f' f' f f z* that is observed in the direction *f' z*. The south pole will, on the same side, be repelled. But on the other side of this plane, the north pole will be repelled, and the south attracted. To ensure this effect, the needle must not be placed above *f' f*, nor below *f' z* or *f' c*. If, instead of presenting a small moveable needle to the conducting wire, there is presented near one of the extremities *f' f*, one of the poles of a powerful magnet, the attraction or repulsion indicated by the needle, will put in motion the galvanic apparatus, and turn it round the prolonged axis of *a b*.

Take, instead of the conducting wire, a strip of copper of the same breadth as the zinc plate, and the only difference from the effect just mentioned, will consist in its being much feebler. The effect is, on the other hand, increased a little, by making the conductor very short. In fig. 6. will be seen a perpendicular section of this arrangement, in the direction of the breadth of the trough. In fig. 7. the same arrangement may be seen in perspective. The conducting plate is represented by *a b c d e f*, and the zinc plate by *c z z f*. The north pole of the needle will, in this arrangement, be attracted towards the plane of *a b c*, and the south will be repelled from the same plane. Contrary effects will take place by an apparatus *e d f*, whose extremities act like the poles of a needle. It must be confessed, however, that only the faces of the two extremities, and not the intermediate parts, possess this analogy.

A moveable galvanic apparatus may likewise be made of two plates,

plates, one of copper and one of zinc, twisted into a spiral, and suspended in the liquid conductor. It is more moveable than the others, but requires to be used with particular caution.

I have not as yet discovered a method of making a galvanic apparatus capable of directing itself towards the poles of the earth. Any apparatus for this purpose must be much more moveable than any I have mentioned.

Notes, by M. Ampere, *of the Communications which he made to the Academy of Sciences.*

Sitting of September 18, 1820.

I reduced the phenomena observed by M. Œrsted to two general facts. I showed that the current which is in the pile, acts on the magnetic needle like that of the conjunctive wire. I described the experiments by which I had established the attraction or repulsion of the whole of a magnetic needle, by the conjunctive wire. I described the instruments which I proposed to construct, and, among others, galvanic spirals. I announced that the latter would produce, in all cases, the same effects as magnets. Afterwards, I entered into some details on the manner in which I conceived the magnets to act; as only owing their properties to electric currents in planes perpendicular to their axis, and upon the similar currents which I allow in the terrestrial globe; in short, I reduced all the magnetic phenomena to effects purely electric.

Sitting of the 25th of September.

I gave a further development of this theory, and I announced the new fact, of the attraction and repulsion of two electric currents, without the intermediation of any magnet; a fact which I had observed in conductors twisted spirally. I repeated this experiment in the course of the sitting.

Sitting of the 9th of October.

I presented to the Academy some experiments, which put in a clear light the identity of action between the conjunctive wires and the close curves, which I conceived like electric currents in planes perpendicular to the line which joins the two poles of a magnet. I showed on two rectilinear electrical currents the same effects, which I had shown in the preceding sitting, on currents in the case of conductors twisted spirally. I read at the same sitting a Memoir, in which I gave the results of some new experiments on the same phenomena, and on the circumstances which produce them. I described the process, which I had since followed, for calculating the effects of electrical currents of a determinate

terminate length, and those of magnets; after that I had determined, by a comparison of the results of experiment with those of calculation, the law of the attraction and repulsion of two infinitely small portions of electric currents. I stated in this memoir all the differences which are established between the attractions and repulsions of electric currents, and those of ordinary electricity, amounting not only to a dissimilarity, but almost to a complete opposition.

SITTING of the 16th of October.

I read a note relative to the interesting experiments of M. Arago, on the magnetising of steel by means of a current, produced by a Voltaic pile. The object of this note was to show, that all the circumstances of that action of electric currents, were conformable with what I had announced on the identity of these currents, and of those which I admit in magnets, and may be regarded as completing the demonstration of it.

SITTING of the 30th of October.

I announced to the Academy that, conformably to my theory of the phenomena which the electric and magnetic currents present, the action of the earth would lead in a plane perpendicular to the direction of the inclination of the needle, the plane of a moveable portion of the conductor of a Voltaic pile, so disposed as to form a circuit nearly closed. I described two sorts of apparatus, the first of which had served me to produce the movement of a conjunctive wire, corresponding to the direction of the needle of a compass, in the horizontal plane corresponding with the line of declination; and the other, that which corresponds to the direction of the inclination of the needle in the plane of the magnetic meridian. I exhibited at the same sitting an instrument, by which there may be turned in a horizontal plane a portion of electric current, the conductor of which is attached to a vertical pivot by the action of another current, an action which conducts it into the situation where these two currents are parallel, and in the same direction.

SITTING of the 6th of November.

I communicated to the Academy a fact relative to the action of conductors twisted in spirals; a fact which I had observed a long time before I discerned the cause of it, which M. Arago had also observed, and whence I deduced—

1st. A very simple means of neutralising the longitudinal effect of an electric current in a conductor twisted spirally, and of reducing the action of it to the transversal effect, which would then be perfectly identical with that of a magnet.

2d.

2d. A law, which I have not verified except in regard to the action exercised by that sort of current, but which may be true in general for each of the infinitely small portions of which electrical currents may be supposed to consist, in order to calculate the effects.

I exhibited at the same sitting an instrument, in which the longitudinal effect of the current, which takes place in a conductor twisted spirally, is neutralized by the prolongation of this conductor, which returns in a right line into the axis of the spiral, from which it is separated by the sides of a glass tube. This instrument, suspended on a pivot like the needle of a compass, possesses all the properties of it when acted upon by a magnet; its extremities represent exactly the poles in the situation in which they ought to be according to the theory.

SITTING of the 13th November, 1820.

I read a note upon the electro-chemical effects of a spiral of iron wire, subjected to the action of the earth alone. The action of the earth directing an electric current as well as it directs a magnet, as I had announced to the Academy in its previous sittings, I thought that this action might, like that of a magnet in the experiment of M. Fresnel, influence the oxidation of an iron wire in water. I therefore plunged under a small glass bell, in a weak solution of chloruret of sodium, the two extremities of an iron wire, which made thirty turns round a paper cylinder, the axis of which was nearly parallel to the variation of the inclination of the needle.

The two wires soon appeared covered with some bubbles; they were much more numerous on the wire which, according to theory, answered to the negative pole of the pile.

During three days which the apparatus remained in action, I several times made the bubbles mount to the top of the bell, so that no more remained on the wires. Every time new ones were produced on the wire which had at first produced most, and remained brilliant until the end of the experiment. The other wire did not present any more, or at least very rarely, since it was oxidized. The apparatus having been accidentally overturned, I was unable to ascertain whether the bubble in the superior part of the bell contained hydrogen, or a greater portion of azote than atmospheric air; or if it was air such as is ordinarily mixed in water, and disengaged from it by the elevation of the temperature of the chamber. On repeating the experiment with the same apparatus, I had only very feeble signs of the electro chemical action. In fact, I have still some doubts as to the existence of that action, which I purpose to clear up by new experiments.

VII. *On the Catenary Curve.* By A CORRESPONDENT.

To Mr. Tilloch.

SIR, — THE following problem relating to the catenarian curve will not be uninteresting to some of your readers, if you can find room for it.

PROB. Of all the catenarian curves that can be formed by suspending different lengths of the same chain of uniform thickness, from two given points placed in the same horizontal line, it is required to determine that one in which the pull at the points of suspension is least.

Let x represent the absciss taken from the vertex, or lowest point of the curve, along the vertical axis; y the corresponding horizontal ordinate; and z the length of chain, or the part of the curve line between the ordinate y and the vertex. Then the weight of the chain z is sustained by the two pulls at its extremities. The pull at the vertex, denoted by a, is horizontal in its direction. The pull at the other extremity of z, denoted by f, is oblique to the horizon; and, by the resolution of forces, it is equivalent to the force $\frac{dy}{\sqrt{dx^2 + dy^2}} \times f$, acting horizontally in contrary direction to a; and likewise to the force, $\frac{dx}{\sqrt{dx^2 + dy^2}} \times f$, directed vertically upwards. Now the equilibrium of every part of the chain requires that the horizontal and vertical forces acting upon it in opposite directions, shall be separately equal to one another: wherefore,

$$\frac{dy}{\sqrt{dx^2 + dy^2}} \times f = a \qquad (1)$$

$$\frac{dx}{\sqrt{dx^2 + dy^2}} \times f = z.$$

From these equations, we get

$$dx : dy :: z : a; \text{ and hence}$$

$$\sqrt{dx^2 + dy^2} = dz : dy :: \sqrt{z^2 + a^2} : a$$

$$\sqrt{dx^2 + dy^2} = dz : dx :: \sqrt{z^2 + a^2} : z; \text{ wherefore}$$

$$dy = \frac{adz}{\sqrt{z^2 + a^2}},$$

$$dx = \frac{zdz}{\sqrt{z^2 + a^2}},$$

and if we now integrate, observing that x, y, and z vanish together, we shall obtain

$y =$

$$y = a \times \log. \frac{z + \sqrt{z^2 + a^2}}{a} \qquad (2)$$

$$x = \sqrt{z^2 + a^2} - a;$$

which are the usual equations of the catenary.

Let ϕ denote the angle between the curve and the ordinate y; thus

$\frac{dy}{\sqrt{dx^2 + dy^2}} = \cos \phi$, and $\frac{dx}{\sqrt{dx^2 + dy^2}} = \sin \phi$; and, on account of the equation (1),

$$a = f \cos \phi \qquad (3)$$
$$z = f \sin \phi.$$

If these values be substituted in the expressions of y and x:

then $$y = f \times \cos \phi \log. \frac{1 + \sin \phi}{\cos \phi} = f \times Q$$
$$x = f \times (1 - \cos \phi) = 2 f \sin^2 \tfrac{1}{2} \phi, \qquad (4)$$

the symbol Q being put for $\cos \phi \log. \frac{1 + \sin \phi}{\cos \phi}$.

Take the fluxion of Q; thus

$$\frac{dQ}{d\phi} = - \sin \phi \log. \frac{1 + \sin \phi}{\cos \phi} + 1: \text{ and because}$$

$$\frac{1 + \sin \phi}{\cos \phi} = \frac{1 + \sin \phi}{\sqrt{1 - \sin^2 \phi}} = \sqrt{\frac{1 + \sin \phi}{1 - \sin \phi}}, \text{ we have}$$

$$\frac{dQ}{d\phi} = - \frac{\sin \phi}{2} \log. \frac{1 + \sin \phi}{1 - \sin \phi} + 1;$$

and, by expanding the logarithms,

$$\frac{dQ}{d\phi} = 1 - \sin^2 \phi - \frac{1}{3} \sin^4 \phi - \frac{1}{5} \sin^6 \phi - \&c. \quad (5)$$

Now Q is equal to zero, both when $\phi = 0$ and $\phi = 90°$: for, in the latter case, although $\log. \frac{1}{\cos \phi}$ is infinite, yet $\cos \phi \log. \frac{1}{\cos \phi}$ is evanescent. As the angle ϕ increases from 0 to $90°$, $\frac{dQ}{d\phi}$ is at first positive till $\sin \phi$ acquires a certain value, determined by the equation.

$$1 = \sin^2 \phi + \frac{1}{3} \sin^4 \phi + \frac{1}{5} \sin^6 \phi + \&c.; \qquad (5)$$

it then vanishes, and afterwards becomes negative. The function Q is therefore susceptible of a *maximum*, which it attains when $\sin \phi$ satisfies equation (5). Now, y being constant in the equation $y = f \times Q$, f will decrease as Q increases; and the first quantity will be a *minimum* when the second is a *maximum*. The *minimum* required is therefore determined by equation (5).

From equation (5) we get

$$\text{Sin}^2\phi = 1 - \frac{1}{3} + \frac{1}{45} + \frac{1}{189} + \frac{11}{11175} + \&c.; \text{ wherefore,}$$

$$\text{Sin}^2\phi = \cdot 8948; \ \sin\phi = \cdot 8335; \ \phi = 56^\circ\ 28'.$$

Again, from the equation $\frac{d\,Q}{d\,\phi} = 0$, or

$$-\text{Sin}\,\phi \log.\frac{1+\sin\phi}{\cos\phi} + 1 = 0, \text{ we get}$$

$$Q = \cos\phi \log.\frac{1+\sin\phi}{\cos\phi} = \frac{\cos\phi}{\sin\phi};$$

consequently, because $f = \frac{y}{Q}$, we have

$$f = y \tan\phi.$$

And hence, from equations (2) and (3), we deduce

$$a = y \sin\phi$$
$$z = y \sin\phi \tan\phi$$
$$x = 2y \tan\phi \sin^2\tfrac{1}{2}\phi.$$

When the catenary has a small inclination to the horizon, the pull is very great; because a very great proportional force acting nearly in the horizon is required to sustain any proposed weight. It is impossible to stretch the chain in a position perfectly horizontal, the force necessary for this purpose being infinitely great. As the angle which the curve makes with the horizon increases, the pull diminishes more on account of the increased inclination, than it increases by the greater length of chain; and this diminution goes on till, at 56° 28′, the *minimum* takes place. Beyond this limit, the pull increases continually, as the length of chain becomes greater.

A. B.

VIII. *On the Compressibility of Water.* *By* Jacob Perkins, *Esq. Communicated by the late Right Hon. Sir* Joseph Banks, *Bart. G.C.B. P.R.S.**

Having believed for many years that water was an elastic fluid, I was induced to make some experiments to ascertain the fact. This was done by constructing an instrument which I call a piezometer, and which is represented in Plate 1, fig. 1. The cylinder, A, was three inches diameter, and eighteen inches long. The end, B, was made water tight by means of a plate which was soldered firmly to it. At the other end, C, a cap was made

* From the Transactions of the Royal Society for 1820, Part II.

to

to screw on and off at pleasure; being also made water tight. The rod or plunger, D, which was five-sixteenths of an inch in diameter, was made to pass through a tight stuffing box, E. On the rod immediately above the stuffing box, was fixed a flexible ring, *a*. A cannon, fig. 2, of a sufficient size to contain the piezometer, was fixed vertically in the earth, the muzzle being left about eighteen inches above ground, and the touch-hole plugged tight. At the mouth a strong cap, A, was firmly screwed on. In the centre of this cap a small forcing pump, B, was tightly screwed, the piston of which was five-eighths of an inch in diameter. There was an aperture, C, in the cap, to introduce a valve for the purpose of ascertaining the degree of pressure. One pound pressure on this valve indicated an atmosphere. The piezometer was introduced into the cannon, and the water forced in until the cap showed signs of leakage; the valve at the same time indicating a pressure of one hundred atmospheres. The piezometer was then taken out of the cannon, and the flexible ring found to be eight inches up the rod, evidently proving the rod to have been forced into the cylinder that distance, showing also a compression of about one per cent. We have seen by repeated experiments, that to be able to produce this degree of compression, three per cent must be pumped into the gun. This fact proves, either that the gun expands, or that the water enters the pores of the cast iron; it is probable both these circumstances contribute to produce this effect.

This experiment was made in America in the year 1819, and before I had time to strengthen my apparatus for the purpose of making further experiments, I was obliged to embark for this country. On my passage, however, I had frequent opportunities of repeating those I had already made, and of making others by a natural pressure. They were as follows. The piezometer, by the assistance of fifty-four pounds of lead attached to it, was sunk in the ocean to the depth of five hundred fathoms, which is about equal to the pressure of one hundred atmospheres. When drawn in, the gauge or ring was found removed eight inches up the rod, indicating, as in the before-mentioned experiment, a compression of one per cent. This experiment was several times repeated, and with the same result.

The next experiment was that of sinking a strong empty porter bottle to the depth of one hundred and fifty fathoms, having first tightly corked and sealed it, in the following manner. Six coverings of cotton cloth, saturated with a composition of sealing wax and tar, were strongly fastened over the cork, by a cord wound round them, directly under the projection at the neck of the bottle. After the bottle had been suffered to remain at the depth mentioned a few minutes, it was drawn up. No water was found

found to have been forced into it, neither was there any visible change at the mouth.

The same bottle was again sunk, and at the increased depth of two hundred and twenty fathoms: when drawn in, it was found to contain about a gill of water; but not the slightest visible change had taken place in the sealing.

The same bottle was now sunk, for the third time, to the still greater depth of three hundred fathoms, and when drawn up, only a small part of the neck was found attached to the line. Its appearance was truly interesting. The bottle was not broken by external pressure, but evidently by the expansion of the condensed sea water, which had found its way through the sealing. Upon examination, it was found that the cork had been compressed into half its length, making folds of about one-eighth of an inch; and that the coverings, consisting of six layers of cloth and cement, had been torn up on one side before the bottle burst. The effect produced upon the cork cannot, we imagine, be accounted for but in one way, viz. that the water, divided into very minute particles, must, by the surrounding pressure of water, have been forced through the coverings, and filled the bottle; that the water thus forced in and condensed, to a great degree, expanded as the pressure was removed by drawing it towards the surface, not only so as to press the cork back into the neck, and, owing to the resistance of the coverings, to compress it half its size, but to separate the neck from the body of the bottle.

Experiment 4. An empty porter bottle, the strongest that could be found, was stopped in the following manner. A cork with a large head was firmly driven into the neck; it was then covered with six layers of fine linen, saturated with a composition of tar and wax; over them was applied a covering of leather, and all perfectly secured by being well bound at the neck. The bottle thus prepared was sunk two hundred and seventy fathoms. When drawn in, it was found perfectly sound, and the sealing unchanged; but filled with water to within an inch of the cork. The coverings were taken off, layer after layer, but no signs of moisture were visible. Had the bottle remained down a sufficient length of time to have completely filled, it would undoubtedly have been broken by the expansion of the water upon being drawn towards the surface, as was the case in the former experiment. It is worthy of remark, that when the water from this bottle was poured into a tumbler, it effervesced like mineral water.

Experiment 5. In this experiment two strong bottles were sunk to the depth of five hundred fathoms. One of them was stopped with a ground glass stopper, and well cemented, then placed in a strong canvass bag. When the bag was drawn in, it

was

was found that the bottle had been crushed into many thousand pieces. The other bottle was very tightly corked; but not having been left down a sufficient length of time, it came up whole, having filled to within one inch and a half. The cork had been driven in, and remained so; but the cementation was unaltered, excepting at the surface, where it had become a little concave.

Being satisfied that the piezometer as first constructed, would not show all the compression, I determined to make one differently modified. The object was to avoid the friction occasioned by the collapsing of the leather upon the rod under such great pressure. The drawing in Plate I, fig. 2, shows another modification of the piezometer, made since I have been in this country. This proves my suspicions to have been correct; since, under the same pressure, it indicated nearly double the compression shown by the former.

This instrument is constructed as follows, fig. 3, being a section of it. It is simply a small tube, A, closed at the end, B, and water-tight. At the upper end, C, the water is allowed to enter through a small aperture, E, closed by a very sensible valve opening inwards. The tube is flattened at D, in order that it may yield to the expansion of the water when taken out of the press.

The experiment with this instrument was made at Mr. Keir's manufactory, in the presence of many scientific gentlemen. The piezometer being perfectly filled with water (the weight of which was accurately known) was put into an hydraulic press, and subjected to a pressure of about three hundred and twenty-six atmospheres. When it wss taken out and weighed, there was found an increase of water amounting to three and a half per cent. This water had been previously boiled, and cooled down to a temperature of forty-eight degrees, and kept at the same temperature during the experiment.

A machine calculated to avoid loss of pressure from destruction of the materials of which it is composed, will be made with all convenient speed. This machine being constructed with metallic stuffings and flexible metallic pistons, will effect a much greater pressure than the hydraulic press, the power of which is limited by the animal stuffing now used. It is probable, a pressure of from two to three thousand atmospheres may be obtained before the metallic piston is destroyed.

It is expected that this machine will be sufficiently accurate to give the exact ratio of the compressibility of water with much greater precision than has hitherto been obtained; but the results of further experiments must be the subject of a future communication.

29, Austin Friars, June 6, 1820.

IX. *Notices*

IX. *Notices respecting New Books.*

A Dictionary of Chemistry, on the Basis of Mr. NICHOLSON'S, *in which the Principles of the Science are investigated anew, and its Applications to the Phenomena of Nature, Medicine, Mineralogy, Agriculture, and Manufactures, detailed. By* ANDREW URE, M.D. Professor of the Andersonian Institution, Member of the Geological Society, &c. &c.

WE hasten to give some account of a work calculated to excite great interest in the chemical world. Its author has been long known as a brilliant and successful public teacher of the science; and peculiarly conversant in its useful applications. His various Memoirs printed in the Transactions of the Royal Societies of London and Edinburgh, as well as in the scientific journals, display much ingenuity joined to patient research; and his Tables on Heat, and the Acids, are now adopted as the standard authorities on their respective subjects.

We learn from the Introduction to this Dictionary, that in the month of June last he was engaged by a London publisher to revise Nicholson's octavo Dictionary for a new edition; but he has not restricted himself to the simple and easy functions of an editor. Dr. Ure has, in fact, re-written three-fourths of the work; and has gratuitously inserted many profound dissertations on the most important and intricate departments of chemistry. The investigations of facts are conducted with candour, acuteness, and logical precision; and the language is perspicuous and elegant. Statements which other writers spread over a page, are communicated by Dr. Ure with perfect clearness in a sentence or two. The volume consists of about 800 pages, in double columns, equivalent to nearly three ordinary octavo volumes. We have no hesitation in affirming, that it contains greater power of original research, than any body of chemical knowledge which has appeared since the Elements of Sir H. Davy. That it was all composed within a period of five months, as the author states in the Introduction, is evident from the references interspersed through it; and gives us a favourable idea of his intellectual resources.

A few extracts will satisfy our readers that we have not been bestowing unmerited praise on this Dictionary; but at present we must be more brief than we could wish.

"INTRODUCTION.

"In this Introduction I shall first present a GENERAL VIEW of the objects of Chemistry, along with a scheme for converting the alphabetical arrangement adopted in this volume, into a systematic order of study. I shall then describe the manner in which

which this Dictionary seems to have been originally compiled, and the circumstances under which its present regeneration has been attempted. This exposition will naturally lead to an account of the principles on which the investigations of chemical theory and facts have been conducted, which distinguish this Work from a mere compilation. Some notice is then given of a Treatise on Practical Chemistry, publicly announced by me upwards of three years ago, and of the peculiar circumstances of my situation as a teacher, which prompted me to undertake it, though its execution has been delayed by various obstructions.

"The forms of matter are numberless, and subject to incessant change. Amid all this variety which perplexes the common mind, the eye of science discerns a few unchangeable primary bodies, by whose reciprocal actions and combinations this marvellous diversity and rotation of existence are produced and maintained. These bodies having resisted every attempt to resolve them into simpler forms of matter, are called *undecompounded*, and must be regarded in the present state of our knowledge as *experimental elements*. It is possible that the elements of nature are very dissimilar; it is probable that they are altogether unknown; and that they are so recondite, as for ever to elude the sagacity of human research.

"The primary substances which can be subjected to measurement and weight, are fifty-three in number. To these, some chemists add the imponderable elements,—light, heat, electricity, and magnetism. But their separate identity is not clearly ascertained.

"Of the fifty-three ponderable principles, certainly three, possibly four, require a distinct collocation from the marked peculiarity of their powers and properties. These are named *Chlorine*, *Oxygen*, *Iodine* (and Fluorine?) These bodies display a pre-eminent activity of combination, an intense affinity for most of the other forty-nine bodies, which they corrode, penetrate, and dissolve; or, by uniting with them, so impair their cohesive force, that they become friable, brittle, or soluble in water, however dense, refractory, and insoluble they previously were. Such changes, for example, are effected on platinum, gold, silver, and iron, by the agency of chlorine, oxygen, or iodine. But the characteristic feature of these archeal elements is this, that when a compound consisting of one of them, and one of the other forty-nine more passive elements, is exposed to voltaic electrization, the former is uniformly evolved at the positive or vitreo-electric pole, while the latter appears at the negative or resino-electric pole.

"The singular strength of their attractions for the other simple

ple forms of matter, is also manifested by the production of heat and light, or the phenomenon of combustion, at the instant of their mutual combination. But this phenomenon is not characteristic; for it is neither peculiar nor necessary to their action, and, therefore, cannot be made the basis of a logical arrangement. Combustion is vividly displayed in cases where none of these primary dissolvents is concerned. Thus some metals combine with others with such vehemence as to elicit light and heat; and many of them, by their union with sulphur, even *in vacuo*, exhibit intense combustion. Potassium burns distinctly in cyanogen (carburetted azote), and splendidly in sulphuretted hydrogen. For other examples to the same purpose, see COMBUSTIBLE and COMBUSTION.

"And again, the phenomenon of flame does not necessarily accompany any of the actions of oxygen, chlorine, and iodine. Its production may be regulated at the pleasure of the chemist, and occurs merely when the mutual combination is rapidly effected. Thus chlorine or oxygen will unite with hydrogen, either silently and darkly, or with fiery explosion, as the operator shall direct.

"Since, therefore, the quality of exciting or sustaining combustion is not peculiar to these vitreo-electric elements; since it is not indispensable to their action on other substances, but adventitious and occasional, we perceive the inaccuracy of that classification which sets these three or four bodies apart under the denomination of *supporters of combustion*; as if, forsooth, combustion could not be supported without them, and as if the support of combustion was their indefeasible attribute, the essential concomitant of their action. On the contrary, every change which they can produce, by their union with other elementary matter, may be effected *without* the phenomenon of combustion. See section 5th of article COMBUSTION.

"The other forty-nine elementary bodies have, with the exception of azote (the solitary incombustible), been grouped under the generic name of *combustibles*. But in reality combustion is independent of the agency of all these bodies, and therefore *combustion may be produced without any combustible*. Can this absurdity form a basis of chemical classification? The decomposition of euchlorine, as well as of the chloride and iodide of azote, is accompanied with a tremendous energy of heat and light; yet no combustible is present. The same examples are fatal to the theoretical part of Black's celebrated doctrine of latent heat. His facts are, however, invaluable, and not to be controverted, though the hypothetical thread used to connect them be finally severed.

"To the term *combustible* is naturally attached the idea of the body

body so named affording the heat and light. Of this position it has been often remarked that we have no evidence whatever. We know, on the other hand, that oxygen, the incombustible, could yield, from its latent stores, in Black's language, both the light and heat displayed in combustion; for mere mechanical condensation of that gas, in a syringe, causes their disengagement. A similar condensation of the combustible hydrogen, occasions, I believe, the evolution of no light. From all these facts, it is plain, that the above distinction is unphilosophical, and must be abandoned. In truth, every insulated or simple body has such an appetency to combine, or is solicited with such attractive energy by other forms of matter, whether the actuating forces be electro-attractive, or electrical, that the motion of the particles constituting the change, if sufficiently rapid, may always produce the phenomenon of combustion.

"Of the forty-nine resino-polar elements, forty-three are metallic, and six non-metallic.

"The latter group may be arranged into three pairs:—

"1st, The gaseous bodies, Hydrogen and Azote;

"2d, The fixed and infusible solids, Carbon and Boron;

"3d, The fusible and volatile solids, Sulphur and Phosphorus.

"The forty-three metallic bodies are distinguishable by their habitudes with oxygen, into two great divisions, the Basifiable and Acidifiable metals. The former are thirty-six in number, the latter seven.

"Of the thirty-six metals, which yield by their union with oxygen salifiable bases, three are convertible into alkalis, ten into earths*, and twenty-three into ordinary metallic oxides. Some of the latter, however, by a maximum dose of oxygen, seem to graduate into the acidifiable group, or at least cease to form salifiable bases.

"We shall now delineate a general chart of chemistry, enumerating its various leading objects in a somewhat tabular form, and pointing out their most important relations, so that the readers of this Dictionary may have it in their power to study its contents in a systematic order."

"Acid Acetic. The same acid which, in a very dilute and somewhat impure state, is called vinegar. The varieties of acetic acids known in commerce are four: 1st, wine vinegar; 2d, malt vinegar; 3d, sugar vinegar; 4th, wood vinegar. We shall describe first the mode of making these commercial articles, and then that of extracting the absolute acetic acid of the chemist, either from these vinegars, or directly from chemical compounds, of which it is a constituent.

* I here regard silica acting as a base to fluoric acid, in the fluosilicic compound; but the subject is mysterious. See Acid (Fluoric).

"A crude vinegar has been long prepared for the calico printers, by subjecting wood in iron retorts to a strong red heat. The following arrangement of apparatus has been found to answer well. A series of cast-iron cylinders, about 4 feet in diameter, and 6 feet long, are built horizontally in brick work, so that the flame of one furnace may play round about two cylinders. Both ends project a little from the brick work. One of them has a disc of cast-iron well fitted and firmly bolted to it, from the centre of which disc an iron tube about 6 inches diameter proceeds, and enters at a right angle the *main* tube of refrigeration. The diameter of this tube may be from 9 to 14 inches, according to the number of cylinders. The other end of the cylinder is called the mouth of the retort. This is closed by a disc of iron, smeared round its edge with clay-lute, and secured in its place by wedges. The charge of wood for such a cylinder is about 8 cwt. The hard woods, oak, ash, birch, and beech, are alone used. Fir does not answer. The heat is kept up during the day-time, and the furnace is allowed to cool during the night. Next morning the door is opened, the charcoal removed, and a new charge of wood is introduced. The average product of crude vinegar called pyrolignous acid is 35 gallons. It is much contaminated with tar; is of a deep brown colour; and has a sp. gr. of 1.025. Its total weight is therefore about 300lbs. But the residuary charcoal is found to weigh no more than one-fifth of the wood employed. Hence nearly one-half of the ponderable matter of the wood is dissipated in incondensable gases. Count Rumford states, that charcoal is equal in weight to more than 4-10ths of the wood from which it is made. And M. Clement says that it is equal to one-half. The Count's error seems to have arisen from the slight heat of an oven to which his wood was exposed in a glass cylinder. The result now given is the experience of an eminent manufacturing chemist at Glasgow. The crude pyrolignous acid is rectified by a second distillation in a copper still, in the body of which about 20 gallons of viscid tarry matter are left from every 100. It has now become a transparent brown vinegar, having a considerable empyreumatic smell, and a sp. gr. of 1.013. Its acid powers are superior to those of the best household vinegar, in the proportion of 3 to 2. By redistillation, saturation with quick-lime, evaporation of the liquid acetate to dryness, and gentle torrefaction, the empyreumatic matter is so completely dissipated, that on decomposing the calcareous salt by sulphuric acid, a pure, perfectly colourless, and grateful vinegar rises in distillation. Its strength will be proportional to the concentration of the decomposing acid.

The acetic acid of the chemist may be prepared in the following modes: 1st, Two parts of fused acetate of potash with one

of

of the strongest oil of vitriol yield, by slow distillation from a glass retort into a refrigerated receiver, concentrated acetic acid. A small portion of sulphurous acid, which contaminates it, may be removed by redistillation, from a little acetate of lead. 2d, Or 4 parts of good sugar of lead, with 1 part of sulphuric acid treated in the same way, afford a slightly weaker acetic acid. 3d, Gently calcined sulphate of iron, or green vitriol, mixed with sugar of lead in the proportion of 1 of the former to $2\frac{1}{2}$ of the latter, and carefully distilled from a porcelain retort into a cooled receiver, may be also considered a good economical process. Or without distillation, if 100 parts of well dried acetate of lime be cautiously added to 60 parts of strong sulphuric acid, diluted with 5 parts of water, and digested for 24 hours, and strained, a good acetic acid, sufficiently strong for every ordinary purpose, will be obtained.

"The distillation of acetate of copper or of lead *per se*, has also been employed for obtaining strong acid. Here, however, the product is mixed with a portion of the fragrant pyro-acetic spirit, which it is troublesome to get rid of. Undoubtedly the best process for the strong acid is that first described, and the cheapest the second or third. When of the utmost possible strength its sp. gravity is 1.062. At the temperature of 50° F. it assumes the solid form, crystallizing in oblong rhomboidal plates. It has an extremely pungent odour, affecting the nostrils and eyes even painfully, when its vapour is incautiously snuffed up. Its taste is eminently acid and acrid. It excoriates and inflames the skin."

"Equivalents Chemical." Under this head Dr. Ure has given a Dissertation on the Atomic Theory, which to us seems to be the best that has yet appeared on this interesting and fundamental doctrine. Had our present limits permitted us, we should have laid it entire before our readers. It shall, if possible, appear in our next. We mean also, as soon as opportunity will permit, to advert to other new views of practical chemistry, opened up by Dr. Ure, and which we consider highly valuable.

The facility of reference would have been improved had the author, where any article occupies a large space, put its whole title at the top of the pages containing it, instead of only the three initial letters.

A Description of the changeable Magnetic Properties possessed by all Iron Bodies, and the different Effects produced by the same on Ships' Compasses. By P. Lecount, Midshipman in the Royal Navy. 8vo, pp. 56.

This appears to be a most useful practicable work. The author corrects several erroneous opinions that have been entertained, and points out a variety of facts which have not before been

been well understood. It has been generally held that vertical bars of iron, which have remained long in that position, acquire a magnetic property, the upper end being a south pole, and the lower a north one. But it now appears that this property is communicated instantaneously, and that the polarity may be reversed by reversing the position of the bar. In southern latitudes the upper end is a north pole.

No seaman should be without this work, which satisfactorily explains several of the phenomena pointed out by Captain Flinders and Mr. Bain in their works on the same subject. The author mentions in an advertisement at the end of his tract, that he had not heard of Mr. Barlow's work on Magnetic Attractions, till his own was at press. We think his pages produce internal evidence of the fact. These two works should accompany each other.

Being no seamen, we may suggest nonsense; but it has occurred to us that advantage might sometimes be found from having a compass aloft, in one of the tops. Whether any system of gimbles could keep a compass steady enough for use, in such a situation, others must determine.

Address of M. Hoene Wronski to the British Board of Longitude, upon the actual State of the Mathematics, their Reform, and upon the new Celestial Mechanics, giving the definitive Solution of the Problem of Longitude.

The author is a foreigner, and the work is a translation from his French. This will sufficiently account for the gallicisms which are found in it. The author came to this country to offer the fruits of his labours and discoveries to the Board of Longitude, and complains of the reception he met with. It is obvious that, allured by the premiums at the disposal of the Board, he put himself to great trouble and expense, and involved himself in difficulties which deserved commiseration,—and, many will be inclined to think, some more substantial remuneration than he has yet obtained, even if his proffered discoveries should prove nugatory. It is natural that a person in his situation should put a high value upon labours which he firmly believes offer the most important results; and we cannot doubt that the Board,—should it even cost some trouble, with, in their estimation, little promise of benefit to science,—will yet be at some pains to ascertain, by an impartial investigation, and by affording the author every facility to demonstrate his problems, the real merits of what he offers. Dr. Young has publicly acknowledged that the author has detected a blunder in his Postscript on Refractions, published in the Nautical Almanack for 1822. This fact shows that, on the subject of refraction at least, the author is not a mere pretender to mathematical knowledge.

Lately

Lately published,

A View of the Agriculture, Manufactures, Statistics, and State of Society of Germany, and Parts of Holland and France. By William Jacob, Esq. F.R.S. 4to. 1*l.* 15*s.*

Gnesenthwait's New Theory of Agriculture; in which the Nature of the Soils, Crops, and Manures is explained, and the Application of Bones, Gypsum, Lime, Chalk, &c. determined on scientific Principles. 5*s.*

Observations on the Construction and Fitting-up of Meeting-Houses, &c. embracing the Method of Warming and Ventilating. By W. Alexander. 4to. 9*s.*

The Botanical Cultivator; or Instructions for the Management of Plants cultivated in the Hothouses of Great Britain. By Robert Sweet, F.L.S. 8vo. 10*s.* 6*d.*

The Elements of Chemistry, with its Application to explain the Phænomena of Nature, and the Processes of Arts and Manufactures. By James Miller, M.D.

The Mental Calculator, being a Compendium of general Rules for the Solution of various Problems in Astronomy; with Explanatory Illustrations. To which is added a Guide to the Constellations. By P. Lovekin. 3*s.*

Treatise on the Principles of Landscape Design, No. 1 to 8. By John Varley. Folio. 5*s.* each.

A Practical Treatise on Perspective, adapted for the Study of those who draw fom Nature. No. 1 and 2. By John Varley. Oblong folio, 5*s.* each.

Numerous Cases illustrative of the Efficacy of Prussic Acid in Affections of the Stomach. By J. Elliotson, M.D. 5*s.* 6*d.*

A Dissertation on the Treatment of morbid local Affections of the Nerves. By Joseph Swan. 8vo. 10*s.* 6*d.*

Illustrations of Phrenology. By Sir G. S. Mackenzie, Bart. 8vo. 15*s.*

A Synopsis of the Diseases of the Eye, and their Treatment. By B. Travers, F.R.S. 8vo. with six coloured Engravings. 1*l.* 5*s.*

A Practical Treatise on Diseases of the Eye. By J. Vetch. 8vo. 10*s.* 6*d.*

Directions for the Treatment of Persons who have taken Poison, and those in a State of apparent Death, with the means of detecting Poisons in Wine; also of distinguishing real from apparent Death. Translated from the French of M. P. Orfila, by B. H. Black, Surgeon.

The Characters of the Classes, Orders, Genera, and Species; or the Characteristics of the Natural System of Mineralogy. By F. Mohs. 8vo. 6*s.* 6*d.*

The

The Climate of London deduced from Meteorological Observations made in the Neighbourhood of the Metropolis. By Luke Howard. 2 vols. 8vo. 1*l*. 5*s*.

No. VII. VIII. and IX. of the English Lakes, each containing four coloured Plates. 4to. 6*s*.; large paper 10*s*. 6*d*. per Number.

Views of the Muscles of the Human Body, drawn from Nature, and engraved by George Lewis, accompanied by suitable Explanatory References; designed as a Guide to the Student of Anatomy, and a Book of Reference for the Medical Practitioner: with 16 Plates, 4to. 1*l*. 11*s*. 6*d*.

The Pharmacopœia of the Royal College of Physicians of London 1809. Literally translated, and the Chemical Decompositions annexed. By G. F. Collier.

A descriptive, diagnostic, and practical Essay on Disorders of the digestive Organs and general Health; and particularly on their numerous Forms and Complications, contrasted with some Acute and Insidious Diseases; being an Attempt to prosecute the Views of Dr. Hamilton and Mr. Abernethy. By Marshall Hall, M.D. F.R.S. E. &c. 8vo. 7*s*.

Introductory Lecture delivered at the Royal College of Surgeons, May 8, 1820. By B. C. Brodie, F.R.S. 8vo. 3*s*. *bds*.

An Arabic Vocabulary and Index for Richardson's Arabic Grammar. By James Noble. 4to. 10*s*. 6*d*. boards.

Preparing for Publication.

The Elements of Oral Language: or, A Dissertation on the Art of Speech, with respect to its Elementary Sounds, and to the Combination of those Sounds in the Current and Rapidity of Discourse: including also a universal Alphabet, intended to express with Precision all the articulate Sounds uttered by the Human Voice, in Connexion with their various Modifications; being the Result of an Attempt to facilitate the Acquisition of Foreign Languages, and to furnish a Mode of correctly expressing our own. By John Freeman.

Practical Observations in Midwifery. By Dr. Ramsbottom. 8vo.

The Principles of Forensic Medicine explained, illustrated, and applied to British Practice. By J. G. Smith, M.D.

An improved new Edition of Mr. S. F. Gray's Supplement to the Pharmacopœias.

The Philosophy of Painting. By Wolstenholm Parr.

X. *Pro-*

X. *Proceedings of Learned Societies.*

SOCIETY OF SCIENCES OF HAERLEM.

This society has renewed its prize question on the utility of fumigation with chlorine gas (oxygenated muriatic acid) for Jan. 1st, 1822—see our 52d Vol., p. 223—and requires, in addition "that a succinct enumeration be given of the cases in which such fumigation has proved effectual in preventing various contagious diseases."

It has also renewed to the same period the question on the gastric juice—See as above.

The following questions have likewise been proposed by this society:

"How far does the physiology of the human body furnish just grounds for believing, or has experience satisfactorily proved, that oxygen gas is one of the most efficacious remedies for recovering persons who are drowned, suffocated, or in a syncope? And what are the most prompt and certain methods to be employed for this end?"

"How far are we acquainted, from the chemical experiments of Vauquelin and others, with the various species of cinchona. 1. What is the different nature and quality of their constituents? 2. To what particular principle should we ascribe its febrifuge powers? 3. What criteria can we deduce therefrom to distinguish the best species, and the best barks used as substitutes? 4. Are any rules to be obtained for preserving the principle, in which consists its febrifuge power, entire in the various preparations of cinchona?"

"Though vaccination has almost every where put a stop to the epidemic small-pox, that disease has re-appeared within these few years here and elsewhere: and a species of variolous pustules having recently shown themselves in some who have been vaccinated, it is required—1. Of what description are these pustules? In what do they differ from real small-pox? Is it the latter that is produced in those individuals who have been previously vaccinated? Does it arise from constitution, from indisposition, from the matter employed in vaccination, or from other circumstances, and how is it to be prevented? 2. What can, with truth, be asserted, with regard to the duration of the preservative virtue of vaccination? Would it prove of service to re-vaccinate on the re-appearance of the disease? Are the methods employed by us for encouraging vaccination sufficient, and do they tend to cause the entire disappearance of the small-pox? In case they are not, what more efficacious means could be adopted?"

"What is the cause why oysters are occasionally prejudicial to

 health?

health? Is it in consequence of a small worm that is found in them? If so, of what species is it, and whereabouts most easily detected? Are oysters subject to it only at certain seasons? Has the venom of oysters any analogy with that which sometimes renders muscles poisonous and unwholesome? What disorders do such oysters and muscles cause? and what are the most efficacious remedies either for averting the evil or for removing it?"

"Why are shrimps sometimes pernicious? How are such as are so to be distinguished? What disorders do they occasion, and what are the best remedies?"

The prize for the best answer to any of these questions is a gold medal, or 150 florins, at the option of the author.

LINNÆAN SOCIETY.

Jan. 16.—A. B. Lambert, Esq. in the Chair. Continuation of the descriptive Catalogue, by Sir T. S. Raffles, was read, of a Zoological Collection made for the East India Company in Sumatra and its vicinity.

Ursa Malayanus. This bear was caught young, and brought up in the nursery among the children. It appears to be a variety of the common bear, and bear of India. It was perfectly tame, and in its habits exceedingly playful. Sir T. mentions, that it was also a brute of taste, which it displayed at the dinner table, where it was a frequent visitor, by refusing to eat any fruit but Mango-steens, or to drink any wine but Champagne!! The only instance in which it was ever seen angry was when there was none of the latter at the dessert! It commonly messed in peace with a dog, a cat, and a lory. The dog was its favourite, and suffered to worry and tease without offence or resentment. The strength of the animal when full grown was, nevertheless, very great; and it could tear up by the roots from the garden a plantain tree of such size as to be almost too large for its embrace!

Moschus, var.) called by the natives *Kauchil.* This little squirrel-like creature is so proverbially cunning, that a Malay, speaking of a clever rogue, says, "he is as sly as a kauchil." Examples are mentioned which show that the comparison is not unfounded. The kauchil, when caught in a trap, pretends to be dead; but should the springe be incautiously loosened, he leaps up and bounds out of sight in an instant! If hunted and sore pressed, he will jump into the branch of a tree, and hang by his teeth, which he thrusts into the wood, while his pursuers run beneath and lose the scent. This cheating character authorizes the proverb.

XI. *In-*

XI. *Intelligence and Miscellaneous Articles.*

OIL GAS.

On Monday the 15th January, a meeting took place at Hull, to consider of the propriety of lighting the town with gas. Considerable discussion occurred as to the comparative merits of gas from oil and gas from coal. It was stated, that the oil gas threw a better light than that from coal, that it required "smaller apparatus, that it was free from the offensive smell so injurious to breath and destructive of comfort, by which coal gas was accompanied; that it did not corrode the pipes, nor tarnish nor discolour polished metals, silks, &c. as coal gas did, and that it was used in Covent-Garden Theatre, in the Argyle Rooms, in Whitbread's brewery, and some other places. One of the speakers alleged on the contrary, that he procured 417 gallons of gas from 11 lbs. of coal, which cost a penny. The coke produced was worth a penny, and the tar worth a penny more, so that he had a profit of 200 per cent. and the gas for nothing. Besides, his gas had produced no offensive smell, and he had not perceived that his pipes (which were of lead) had been corroded.—A letter was read, which observed, that 1000 feet of oil gas would produce a light equal to 3333 feet of coal gas. It appears that the Emperor Alexander is lighting up his palace at St. Petersburgh with oil gas. The meeting unanimously agreed to resolutions in favour of gas from oil.

ON THE SOLAR SPOTS*.

In the 49th volume of the Philosophical Magazine, p. 182, I find an article upon the solar spots of 1816, by Mr. Mosely, of Winterdyne House, Worcestershire, upon which, and his method of observing them, I have to make a few remarks.

He states that the sun's image was received upon a paper screen, on which was drawn a circle of the same diameter as that produced by the sun's rays, when falling upon it with the focus of the eye-glass properly adjusted; and that across the circle were drawn three lines, one exactly perpendicular to the horizon, another inclining 8 *deg. westward, representing the axis of the sun*, and a third at right angles to the axis, *representing the equator*, &c. &c. Now, as the sun's axis, *when he is on the meridian*, will only coincide with that meridian *twice a year*, viz. about the beginning of September, when his long. is from 5 sec. 8 deg. to 5 sec. 12 deg. and in the latter end of February and beginning of March, when his long. is from 11 sec. 8 deg.

* From New Monthly Magazine for January 1821.

to 11 sec. 12 deg. and will twice a year decline from it the greatest possible quantity, being when his long. is 2 sec. 10 deg. and 8 sec. 10 deg. or in the beginning of June and December, in the first case to the west, and in the second to the east; I should conceive that for such observations, a new projection of the sun's disc, axis, &c. is highly necessary for at least every week or ten days, as in that time there will be a sensible difference in the inclination. I also conceive, that as we cannot always be sure of an observation when *he is on the meridian*, a telescope mounted on a polar axis will be found a proper instrument, as by following the sun's motion in the parallel he describes on any particular day, the projection will, if rightly constructed, be correct at any distance from the meridian, provided the motions of the instrument are good, and it is exceedingly well mounted. I may also further observe, that during the period of observations in question from September to November, the sun's axis declined from *about* 0 *deg.* 10 *min. W.* to *about* 3 *deg.* 45 *min. E.* supposing him *upon the meridian*, instead of being constantly 8 *deg. westward*, as stated by Mr. M.; and consequently those observations are *absolutely wrong*, *if the tables are right*, as to the *position of the axis and direction of the spots.*

I recommend to such as speculate upon these curious phænomena, after observing the sun's image upon paper for the purpose of tracing the places and directions of the spots, to examine them by means of a dark glass applied to their telescope, before they describe *their shape*, as I am convinced from long and repeated observations, that their true appearance is much better seen that way than the other. They would also do well to remember, that a telescope which produces erect vision, when looking in it, inverts the images when received upon paper, and that if inverted vision is produced by looking in the instrument, erect objects will be seen upon the paper, but the right and left edges of an object will change places, when the observer's back is turned upon it, as it must be in such observations.

I prefer a dark glass, or, what is much better, a green and a red one united in one cell, because the penumbra or lighter side of a spot is seen perfectly defined, and smaller spots which are nearly invisible by the other method, may be distinctly seen and counted if necessary by this. Indeed in a favourable atmosphere with no larger instrument than Mr. M.'s, and a very moderate power, I have not only done so, but have seen also the brighter spots, or fæculæ, or bright lumps, behind which the dark ones seem to lie when near the limb, as well as the innumerable small dark points which cover the whole face of the sun, when by receiving the image on paper I could not see them at all.

The *conversion of dark spots containing nuclei, into bright ones*

ones called fæculæ, seems to consist in their being hidden behind the bright spots or lumps of shining matter I have before mentioned, when going off the western limb, and the contrary takes place when entering on the eastern limb, while in the middle of the disc there are rarely, if ever seen, any bright spots or lumps of shining matter, probably from the more intense light of the sun's centre then rendering them invisible.

It is somewhat curious, that although the author quotes Mr. Adams, upon the variable paths of the spots, he has not discovered, in his observations, that these varied paths arise partly from the different inclination of the sun's axis to an azimuth circle, at different hours of the day, on account of the earth's diurnal motion, and partly from the variable inclination of the sun's axis to an azimuth circle, arising from the earth's annual motion: even under the influence of both these causes, the same spot which at sunrise appears to advance from the *southern to the northern limb*, will, in consequence of the sun's apparent motion, appear to advance from the *northern to the southern limb* at sunset: but if we confine ourselves to observations upon the meridian, that part of the phænomena depending upon the earth's annual motion, alone remains, and the spot will travel *northward* when the axis inclines *eastward*, and *southward* when it inclines *westward*. When observed out of the meridian, their path is, in strictness, influenced by both these causes. Their curvilinear direction results from our alternately seeing a little more of the sun's northern, or southern polar regions, when the axis is least inclined, by which means they are convex to the sun's north and south poles by turns, in spring and autumn; this curvature is, therefore, a consequence of the earth's annual motion. The Author's remark, that "*these irregularities are of rare occurrence*," is, therefore, the result of a very slender acquaintance with these subjects. A CORRESPONDENT.

RHUBARB.

By a late analysis made by Mr. Brande on the finest Russian rhubarb, it appeared to contain

Water	8·2
Gum	31·0
Resin	10·0
Extract, tan, and gallic acid	26·0
Phosphate of lime	2·0
Malate of lime	6·5
Woody fibre	16·3
	100·0

Journ. of Science, No. 20.

MAG-

MAGNETISM.

The following observations, on the dip of the needle and the intensity of the magnetic force, have been collected and calculated by Professor Hansteen:

	Dip.	Intensity.
Peru	0°·0	1·000
Mexico	42·10	1·3155
Paris	68·38	1·3482
London	70·33	1·4142
Christiana	72·30	1·4959
Arendahl	72·45	1·4756
Brassa	74·21	1·4941
Hare's Island	82·49	16·939
Davis' Straits	83·8	1·6900
Baffin's Bay	84·25	1·6685
———	84·39	1·7349
———	84·44	1·6943
———	85·54½	1·7383
———	86·9	1·7606

Edin. Journal.

METEORIC STONE.

An aërolite which fell on the 13th of October 1820, near Kostritz in Russia, has lately been analysed by Stromeyer, and found to contain

Silica	38·0574
Magnesia	29·9306
Alumina	3·4688
Protoxide of iron	4·8959
Oxide of manganese	1·1467
Oxide of chromium	0·1298
Iron	17·4896
Nickel	1·3617
Sulphur	2·6957
	99·1768

Journ. of Science, No. 20.

ANTIDOTE AGAINST CORROSIVE SUBLIMATE.

In the course of his experiments on gluten, Dr. Taddei found, that wheaten flour and gluten reduced corrosive sublimate to the state of calomel, and that considerable quantities of a mixture of flour or gluten, with corrosive sublimate, might be taken by animals without any injurious effects. In this way, fourteen grains of sublimate were given in less than twelve hours to rabbits and poultry without injury, whereas a single grain would have been fatal, if taken alone. In order to render a grain of

of the corrosive sublimate innocent, 25 grains of fresh gluten, or 13 of dry gluten, or from 500 to 600 grains of wheaten flour, are necessary.

REDUCTION OF MURIATE (CHLORIDE) OF SILVER.

The use of nitrate of silver in laboratories, as a test for the muriates causes a quantity of muriate of silver to be collected, which is usually reduced to the metallic state by fusion with potash—a process generally accompanied with a loss of silver. The following way is more economical.

Take a clean zinc or iron vessel, or else a glass vessel, with pieces of clean iron or zinc in it; cover it with water, and add the muriate of silver with a little sulphuric or muriatic acid. The reduction soon begins, and offers a very curious sight, particularly when the muriate is in lumps; it begins on the surface, and extends over the whole in the form of ramifications, and penetrates the inner part, so that in less than an hour considerable lumps of the muriate of silver are reduced. Some heat is generated in the process which assists the reduction; or if it goes on slowly, the mixture may be warmed. Wash the reduced metal with a little muriatic acid—*Annales de Chimie* xiv. 319.

THE FRENCH FLOCK OF CASHMIRE GOATS.

The flock, consisting of 175, imported into France in 1819, and placed at the north-east of Toulon, has been removed to a more congenial climate at St. Omer, near Paris. The kids from this flock are abundantly covered with magnificent down of which the Cashmire shawls are made; and they are superior in strength and appearance to the indigenous kids of the same age; which leaves no doubt of success from the naturalization.

THE STATUE OF MEMNON.

The Russian Ambassador at the Court of Rome has received a letter from Sir A. Smith, an English traveller, who is at present at the Egyptian Thebes. He states, that he has himself examined the celebrated statue of Memnon, accompanied by a numerous escort. At six o'clock in the morning he heard very distinctly the sounds so much spoken of in former times, and which had been generally treated as fabulous. "One may," he says, "assign to this phenomenon a thousand different causes, before it could be supposed to be simply the result of a certain arrangement of the stones." The statue of Memnon was overturned by an earthquake; and it is from the pedestal that this mysterious sound is emitted, of which the cause has never been ascertained, and which was denied merely because it was inexplicable.

ANTARC-

ANTARCTIC CONTINENT.

In our last volume (p 93.) we announced the discovery of this land by Captain Smith, of the William of Blythe, who named it *New South Shetland*; not, to be sure, a very correct name, as there is no other *South Shetland*. In consequence of this discovery, the Admiralty sent out the Conway, Captain Basil Hall, to explore the coasts and procure whatever information may be attainable. Advices have since been received of the arrival of the Conway at Rio Janeiro, on her voyage out.

ARCTIC LAND EXPEDITION.

The last accounts from this party are dated in January 1819. They were then in winter quarters at Cumberland House. The temperature 30° below zero, but owing to the dryness of the atmosphere, less unpleasant than the cold wet weather in England. The hunters bring them moose deer and buffaloes, and the rivers and lakes abound with fish.

NEWLY DISCOVERED ISLANDS.

The Swedish journals announce that Major Graner, who set out last year to explore in the South Sea a new route for ships from Chili to the East Indies, has discovered a group of islands hitherto unknown to mariners; but these journals do not mention either their longitude or latitude. He has named the largest of the group Oscar Island.

CHAIN CABLES USEFUL CONDUCTORS OF LIGHTNING.

"Saugor, May 28, 1820.

"A little before 4 o'clock P.M. yesterday, a severe squall from the N.W. commenced, accompanied by torrents of rain, tremendous crashes of thunder, and lightning most awful. At 20′ past 4, the lightning struck the fore royal-mast of the Exmouth, and shivered the mast to the gun-deck in a thousand pieces; struck down and dreadfully burnt several of the crew, and most providentially was conducted out of the hawseholes by the attractive powers of the iron chain cables, by which she was moored; to which fortunate circumstance is entirely to be attributed the preservation of the ship from blowing up, her hold being full of saltpetre."—*Madras Paper, June* 23.

MAMMOTH GOURD.

A gourd was cut some time ago in the garden of H. P. Tozer Aubrey, Esq. of Broomhall, near Oswestry, which, by some peculiar management in the culture, attained the weight of 113 pounds.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To John Sadler, of Penlington-place, in the parish of Lambeth, in the county of Surry, gent. for his new or improved method or process of manufacturing carbonate of lead, formerly denominated ceruse, but now commonly called white lead.—Dated 3 January 1821.—6 months allowed to enrol specification.

To John Leigh Bradbury, of Manchester, in the county Palatine of Lancaster, gent. for his new mode of engraving and etching metal rollers used for printing upon woollen, cotton, linen, paper, cloth, silk, and other substances.—9 Jan.—6 months.

To Robert Salmon, of Woburn, in the county of Bedford, esq. for certain improvements in the construction of instruments for the relief of Hernia and Prolapsus, which instrument so improved he denominates Scientific principled, variable, secure, light, easy, elegant, cheap and durable Trusses.—15 Jan.—6 months.

To John Frederic Daniell, of Gower-street, Bedford-square, in the county of Middlesex, esq. for certain improvements in clarifying and refining sugar.—15 Jan.—6 months.

To Abraham Henry Chambers, of Bond-street, in the county of Middlesex, esq. for his improvement in the manufacture of building cement composition stucco or plaster, by means of the application and combination of certain known materials hitherto unused (save for experiment) for that purpose.—15 Jan.—6 months.

To Charles Phillips, of Albemarle-street, Piccadilly, in the county of Middlesex, commander in our Royal Navy, for his certain improvements in the apparatus for propelling vessels, and improvement in the construction of vessels so propelled.—19 Jan.—6 months.

NEW COMET.

At the Royal Observatory at Paris a new comet was discovered on Sunday the 21st of January, in the constellation *Pegasus*, near the star marked *Gamma*. The comet is not visible to the naked eye.

LUNAR OCCULTATION OF JUPITER.

Aberdeen, Jan. 11, 1821.

Sir,—In my letter which you had the goodness to publish in the last number of your valuable Magazine, I observe two errors, which I imagine I have committed in the hurry of copying from my notes. You will much oblige me by inserting in your next the following corrections of them.

Page 435, line 1st, *for* apparent time, *read* mean time.
——— — 13, *for* 42″·3 *read* 43″·3.

George Innes.

 SOLAR

SOLAR ECLIPSE.

During the eclipse on the 7th of September, M. Necker of Cologny, near Geneva, had two delicate thermometers suspended about four feet from the ground; one of them exposed to the direct rays of the sun, the other near the first, but in the shade, on the north side of a tree. The following are some of the results:

H. M.			In the Sun.		In the shade.
At 1	0	..	96° Fahr.		69·1 Fahr.
1	30	..	97·25	..	70·25
2	0	..	84·90	..	68·00
2	35	..	69·12	..	65·18
2	55	..	78·10	..	65·75
3	15	..	88·25	..	68·00
4	10	..	90·5	..	70·25

Maximum in the sun 97·25—minimum 69·12.
——— in the shade 70·25, min. 65·18.
Difference of the maxima 27°.
——— of the minima 4°.

BAROMETRIC OBSERVATIONS.

Leighton, January 13, 1821.

Dear sir,—I send you the observations made at this place on Monday the 8th instant.

	Bar.	Ther. att.	Ther. det.	Wind.
8h	28·942	32	36½	
9	28·944	32	37	
10	28·943	33	37	S.W.
11	28·943	33½	37	
12	28·927	34	38	E.

I have also received the observations of Col. Beaufoy, as under:

	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
8h	28·705	34·3	38	E.S.E. light	Foggy
9	28·705	34·3	38	E.S.E. fresh	Do.
10	28·691	34·3	38	E. by S. fresh	Fine
11	28·688	34·6	38·5	E. moderate	Fine
12	28·665	34·6	40	E. moderate	Cloudy

I have also been favoured by Col. B. with the result of his calculations on the difference of altitude of our instruments. I think it may be adviseable to make a few more observations on the

the second Monday in the ensuing months, before the results are published. I may however say that a steady uniformity in the results must not be expected, under different circumstances of distance, and direction and strength of wind; and there may be some laws at present unknown that influence the height of the mercury. It is hoped that a few difficulties or anomalies in the onset may not induce us to abandon the pursuit. Before any regular list of the height of towns, rivers, hills, &c. can be published, it is highly desirable to settle the point of *zero*; and as a change of signs + and — will be troublesome to the general reader, and as the surface of the sea at low water, spring tide, has been used for such a standard, it may be right to adopt it as sufficient to express every part of dry land in the country. Considering London as the most important part of these kingdoms, it will be desirable to ascertain the correct height of some few public and accessible points in the metropolis above this zero.

Perhaps the best way to obtain this object would be to procure a correct section of the river Thames from London to Sheerness. At present I am not aware of any such section, although it is a river of more consequence than any other in Great Britain.

If any of your readers should know of any such section, either of the whole or part of the river, a reference to it will be very acceptable. I am, dear sir, yours truly,

B. Bevan.

Blackwater, Hants, Jan. 11, 1821.

To Mr. Tilloch.

Sir,—In compliance with the wish of Mr. Bevan, I send you the state of the barometer and thermometer on the 8th instant at this place. I unavoidably lost the observations at 8 and 9 o'clock, but suppose it of no material consequence. At a rough estimation, the cistern of the barometer may be about 260 feet above the level of the sea. I cannot be certain to a few feet.

I am, sir, your obedient humble servant,

Geo. D. Bins.

Jan. 8. 1821.			Barom.	Therm. attached.	Therm. detached.	Wind.
	H.	M.				
A.M.	9	30	29·09	45·75	39·50	E. gloomy.
	10	0	29·08	46·5	39·50	
	11	0	29·07	48·	40·	*Cirrhi, cirrostrati & cirrocumuli.*
	12	0	29·05	49·5	40·	Gloomy:

Geocentric place of PALLAS *from Jan.* 31 *to July* 30, 1821, *by M.* STAUDT *of Gottingen.*

Midnight at Gottingen.	Æ in time.	N Declination.	Midnight at Gottingen.	Æ in time.	N Declination.
	h ′ ″	° ′		h ′ ″	° ′
Jan. 31	16. 5.20	4. 6	May 3	16.38. 8	23.22
Feb. 4	10.12	4.40	7	35.20	24. 1
8	14.52	5.16	11	32.20	24.35
12	19.16	5.55	15	29. 4	25. 4
16	23.28	6.37	19	25.44	25.29
20	27.20	7.21	23	22.20	25.47
24	30.56	8. 7	27	18.52	26. 1
28	34.16	8.55	31	15.28	26. 9
Ma ch 4	37.20	9.46	June 4	12.12	26.11
8	40. 0	10.38	8	9. 0	26. 9
12	42.20	11.33	12	6. 0	26. 1
16	44.20	12.28	16	3.16	25.48
20	46. 0	13.25	20	0.44	25.31
24	47.12	14.23	24	15.58.32	25.10
28	48. 4	15.22	28	56.40	24.45
April 1	48.32	16.21	July 2	55. 4	24.16
5	48.36	17.19	6	53.48	23.45
9	48.16	18.17	10	52.56	23.11
13	47.32	19.14	14	52.20	22.35
17	46.20	20. 9	18	52. 8	21.57
21	44.48	21. 2	22	52.16	21.17
25	42.56	21.52	26	52.40	20.36
29	40.40	22.38	30	53.28	19.54

Geocentric place of Juno *from May* 5 *to October* 20, 1821, *by M.* Nicolai *of Mannheim.*

Midnight at Mannheim.	Æ. in time.	S. Declination.	Midnight at Mannheim.	Æ. in time.	S. Declination.
	h ′ ″	° ′		h ′ ″	° ′
May 5	20.20.34	5.46	Aug. 1	19.52.23	5.17
9	22.33	5.26	5	49. 2	5.43
13	24.14	5. 7	9	45.52	6.10
17	25.38	4.49	13	42.55	6.39
21	26.43	4.31	17	40.15	7. 9
25	27.28	4.15	21	37.55	7.39
29	27.53	4. 1	25	35.56	8. 9
June 2	27.57	3.47	29	34.21	8.39
6	27.40	3.36	Sept. 2	33.10	9. 9
10	27. 1	3.27	6	32.25	9.38
14	26. 1	3.19	10	32. 6	10. 7
18	24.39	3.14	14	32.14	10.34
22	22.56	3.12	18	32.48	11. 0
26	20.52	3.12	22	33.48	11.25
30	18.29	3.14	26	35.13	11.4
July 4	15.48	3.20	30	37. 3	12.1
8	12.52	3.28	Oct. 4	39.17	12.3
12	9.42	3.40	8	41.55	12.40
16	6.22	3.54	12	44.54	1 3. 8
20	2.55	4.11	16	48.14	13.19
24	19.59.24	4.31	20	51.54	13.32
28	55.52	4.53			

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 129 feet.

	Morning, 8 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Mean Tempr. by Six's	Depth of Rain.	N° of Days.	
1820.	Barom.	Ther.	Barom.	Ther.	Ther.	Inch. 100	Rain or Snow.	Fair.
January.	29·763	29·355	29·776	30·097	30·532	2·30	14	17
February.	29·883	38·655	29·875	39·069	40·310	1·40	12	17
March.	29.761	39·355	29·762	39·615	41·451	0.50	10	21
April.	29·794	45·500	29·795	44·533	47·353	0·90	6	24
May.	29·632	49·516	29·60	47·870	50·741	5·20	19	12
June.	29·838	55·066	29·837	52·966	55·533	1·60	13	17
July.	29·844	57·549	29·788	55.580	53·322	1.80	9	22
August.	29·621	55·645	29·627	53·590	56·806	2·20	13	18
September.	29·792	51·100	29·777	50·060	52·633	1·20	12	18
October.	29·499	43·193	29·480	42·742	44·419	2.50	12	19
November.	29·749	40·633	29·764	40·633	42·133	1·70	11	19
December.	29·877	39·032	29·883	39.129	39·483	2·20	16	15
Average of the year.	29·754	45·388	29·747	44·659	46·724	23·50	147	219

ANNUAL RESULTS.

MORNING.

Barometer.			*Thermometer.*	
Observations.	*Wind.*		*Wind.*	
Highest, 9th Jan.	SW.	30·88	26th June, W.	68°
Lowest, 17th Oct.	NW.	28·58	18th Jan. NW.	1°

EVENING.

Highest, 8th Jan.	SW.	30·88	25th June NW.	67°
Lowest, 17 Oct.	NW.	28·66	18th Jan. NW.	9°

Weather.	*Days.*	*Wind.*	*Times.*
Fair	219	N. and NE.	19
Rain or Snow	147	E. and SE.	97
		S. and SW.	67
	366	W. and NW.	183
			366

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 18th Jan. . . . Wind NW. one below Zero.
Hottest, 26th June . . . . Wind NW. . . . . . 79°
Mean Temperature for 1820 . . . . 46° 7247'

RESULT OF TWO RAIN GAUGES. In. 100

Centre of the Kinfauns Garden, about 20 feet above the level of the Sea	23·5
Kinfauns Castle, 129 feet,	18·5

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1820.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Dec. 15	10	34·	29·86	Cloudy
16	11	36·	29·65	Ditto—snow P.M.
17	12	36·5	29·65	Ditto
18	13	42·	30·	Ditto
19	full	48·5	30·05	Rain
20	15	42·5	30·15	Fine
21	16	49·	29·80	Cloudy
22	17	46·	29·90	Ditto
23	18	41·	29·90	Ditto
24	19	34·5	29·85	Ditto
25	20	35·	29·90	Stormy
26	21	34·	29·77	Ditto
27	22	31·5	29·90	Cloudy—storm at night
28	23	30·5	30·	Stormy
29	24	29·5	30·	Ditto
30	25	30·	30·	Fine
31	26	32·	29·90	Cloudy
1821.				
Jan. 1	27	31·5	29·80	Ditto
2	28	33·5	29·66	Ditto
3	29	30·5	29·40	Ditto
4	new	30·5	29·80	Ditto
5	1	32·	29·25	Ditto—heavy fall of snow P.M.
6	2	34·	29·10	Ditto—Rain A.M.
7	3	36·	29·23	Rain
8	4	38·	29·15	Cloudy
9	5	39·	29·85	Ditto—heavy rain A.M.
10	6	38·	29·04	Ditto
11	7	38·5	29·18	Ditto
12	8	46·	29·05	Fine
13	9	48·	29·30	Cloudy
14	10	40·5	29·60	Ditto

METEORO-

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For January 1821.

Days of Month. 1820.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Dec. 27	30	31	30	29.95	Cloudy
28	30	32	30	·99	Fair
29	25	27	28	·99	Cloudy
30	26	28	25	30·01	Cloudy
31	23	29	29	29·95	Fair
Jan. 1	30	30	30	·91	Cloudy
2	25	27	25	·70	Cloudy
3	28	31	29	·41	Cloudy
4	25	30	30	·45	Cloudy
5	30	31	30	·30	Cloudy—a fall of
6	35	41	37	·23	Foggy [snow dur-
7	35	35	35	·31	Foggy [ing the ngt.
8	37	41	37	·20	Cloudy
9	38	42	38	·10	Foggy
10	39	44	40	·23	Cloudy
11	41	41	41	·20	Rain
12	44	50	45	·45	Fair
13	45	50	48	·62	Fair
14	40	43	36	·65	Cloudy
15	36	38	44	30·06	Rain
16	45	49	38	29·90	Fair
17	35	47	45	30·21	Cloudy
18	45	51	47	·33	Cloudy
19	46	49	47	·41	Cloudy
20	42	49	40	·45	Fair
21	32	45	39	·72	Fair
22	37	41	40	·72	Cloudy
23	35	41	34	·85	Fair
24	32	34	30	·78	Foggy
25	35	42	38	·70	Foggy
26	37	40	37	·67	Cloudy

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
10th January 8 o'Clock M. Barom. 29·230 Ther. attached 46° Detached 39
— — — 9 — — — — ·232 — — — 46 — — 40
— — — 1 — — — — ·238 — — — 44 — — 44

The Barometer is about 71 ft. 3 in. above the mean level of the sea computed by Mr. Bevan, or 23 ft. 3 in. above the pavement in the Strand, near Norfolk-st.

XII. *On the best Means for conducting meteorological Observations in different Places and Climates, so as to produce some Uniformity in the Modes of obtaining and summing up the Results.* By LUKE HOWARD, *Esq.*

To Mr. Tilloch.

MY OLD AND RESPECTED FRIEND,—I HAVE to thank thee for the care and delivery of a letter on Meteorological Subjects, addressed to me from Italy; and in doing this to claim a little space in the pages of the Philosophical Magazine, so often usefully devoted to this branch of science, for the purpose of giving publicity both to the most material parts of the letter, and to a few observations I shall have to make upon it, and which I have no doubt will by this means promptly reach its author.

The gentleman who favours me with this communication, and who subscribes himself "CASTELLANI, Inspecteur dans le Corps de Ponts et Chaussées à Turin," has been led by the duties of his profession to pay a regular attention to meteorological observations; and in attempting to compare his own with those of other observers, in distant climates, has been stopped by the obstacle so generally experienced by the cultivators of this infant science, *the variety which prevails in the mode of obtaining and summing up results.* As it is not at all probable that men of science in different countries will long continue to submit to the glaring absurdity of being thus, as it were, barbarians to each other, in a branch of their common pursuits which promises, by due culture, to become both fruitful and respectable, I think it but justice to this gentleman here to state the principles on which *he* thinks the common system should be founded: which are these:

The *Meteorological year* to begin with the vernal equinox: by this means, the six summer months are made to form the first division, and the six winter months the second; for the purpose doubtless, though he does not mention it, of *contrasting* the mean temperature, rain, &c. of the two seasons; and of each of them with the same in other years.

The *subdivisions* to be of *ten days* each. The author thinks a mean result founded on the *month* comprehends too many of the daily observations, and that of the *week*, too few; while results taken on every ten days would, in his opinion, give the course of temperature, in particular, in a more perspicuous manner. Each of the *four seasons* in this case would consist (with the necessary intercalary additions) of *nine decades* of days.

A more important object still with him is, the proper division of the day of 24 hours: for he thinks we lose much in making our

neglect and indifference to encourage and promote any emendation or extension devised in seeking out the ramifications of theory subordinate to practice. Where then is the consistency, in what is contained the policy of the immediate supporters of these schemes, or the engagements formed therein with them? If indeed they are to be identified with a character for disinterestedness, liberality and ingenuousness, and assuredly connive not at the intervention of stratagem, let them cease longer to hold out beacons deceptive, as well as those false lights that dazzle in their presence, but do not inform and direct the mind of the beholder, if all this be through a cause inadvertently, and not of design to impose on or delude the unwary. In glancing over these observations, it is likely they will be accounted matter of assertion unsupported by testimony of fact: that this may not supervene, I shall recognise, though among several, one circumstance only as of recent occurrence in exemplification, and to show that they have not sprung out of a feeling of invective or prejudice, and as in the sequel there will be cause still to condemn the selfish and mercenary spirit by which such of these public bodies are actuated.

In the course of reference made to me on the subject of contingent property, I was lately requested to report upon the value of three policies of assurance, effected in certainly a very popular and flourishing concern: I allude not to the Equitable, for there in all its affairs we find a conscientious and scrupulous regard paid to the equitable claims and interest of all its members indiscriminately alike: thus a true parent of nature marks no distinction in the being of a common family and stock.

We will now briefly state the terms and dates of these policies when effected; they are as under—the life now in forty-first year of age:

		Prem. paid.	
1.	.. 500	.. 12*l*. 5*s*. 5*d*.	.. 1806
2.	.. 500	.. 12 15 5	.. 1808
3.	.. 1000	.. 26 2 6	.. 1809
	2000	51 3 4	

In the proposed consideration here it is necessary merely to remark, that upon a direct appeal of the party at the office to ascertain the sums that would be returned in lieu of the claims existing on a surrender being made conformably with the usual practice—they were the following nominally:

1 .. 30*l*. 2 .. 23*l*. 3 .. 40*l*.

However, it may be replied by those in the conduct and management of the offices, that the sum the directors may, in caprice, think proper and choose to offer when they cancel a policy,

Six's thermometer, which is commonly graduated by Fahrenheit's method, he must have new modelled the scale to his purpose. On the whole, I must refer this gentleman, and the readers of the Philosophical Magazine in general, to the work I have already mentioned, for much that might be here said in reference to the contents of this letter, the most material part of which I have extracted. It may be observed, as a general proposition, that nothing is likely to be gained by departing from the ordinary mode of publishing Meteorological Tables every month. The European calendar is equally applicable in all parts of the globe; but seasons differ, and are even reversed by the position of the place of observation in another hemisphere. About the polar circle, again, they have a long severe winter and a short warm summer, without spring or autumn: within the tropics, two seasons which approach more nearly in their character to a wet spring and a dry autumn, both indeed at a temperature much higher than we experience in these middle latitudes. As the *four seasons* exist only in *these*, it is here alone that they can be compared in the method proposed: other modes must be adopted for the hot and cold climates, and it is nearly indifferent in what portions the observations are *produced*, provided the method of summing up the results be properly adapted to the climate, the natural divisions of which should in every case be studied and adhered to. There is, I think, much merit in the specimen given by M. Castellani, of his method, in the *Bibliothcque Universelle** : but to me, who have seen it for the first time only within these few days, the same difficulty presents itself in attempting to compare it with my own, of which *he* complains—a new *method* and new *measures* to be studied and comprehended, before the *matter* can be come at for the intended use. The use of *curves* (I repeat it as being the most essential part of my letter) for the purpose of expressing every thing in meteorology that is subject to *measures*, would do away at once so much of this difficulty, as to render it easy to proceed in common with our respective observations, and compare them in detail, as well as in result, at a glance; at least until the great object of a uniform measure for all civilized nations be satisfactorily accomplished. The data in figures should however in this case be required along with the graphical representations, or should at least be kept in readiness to be produced as vouchers of their accuracy.

I am respectfully thy friend,

Tottenham, 23d of first month, 1821. LUKE HOWARD.

* Vol. ii. p. 232, 1819.

XIII. *Additional Observations on the Use of Lactucarium, or Lettuce Opium; particularly in a Case of Cynanche Laryngea, or Croup.* By ANDREW DUNCAN, *Sen. M.D. and P.E. &c.*

Edinburgh, the 14th of October 1820.

SEVERAL years have now elapsed since I recommended to the attention of medical practitioners, a soporific medicine, prepared from the common garden lettuce, the *Lactuca sativa* of Linnæus. In a paper, first published in the Memoirs of the Caledonian Horticultural Society, about ten years ago, I described what I considered as the best method of preparing it; and I gave to the article thus prepared the name of *Lactucarium*, that there might be no similarity in sound between the appellation given to this sedative, and to that which is obtained from the *Papaver somniferum*, and which is universally known by the name of Opium, a name derived from its being the coagulum of a milky juice. For although the inspissated milky juice obtained from the lettuce, the *Opium Lactucæ*, as it might be called, possesses many of those medical properties which have long been known to exist in the milky juice of the poppy, the *Opium papaveris*, particularly in procuring sleep, yet it differs from the opium obtained from the poppy, in several particulars. And from these differences *Lactucarium* can be exhibited with success, in many cases where opium is altogether inadmissible.

Since the publication of my first observations on *Lactucarium*, the attention of many others has been turned to this article. In the same work in which my observations were published, the Memoirs of the Caledonian Horticultural Society, several interesting communications have appeared respecting it; particularly by Mr. John Henderson of Brechin, Mr. Archibald Gorrie of Rait, and others. To these gentlemen the public are indebted for improved methods of collecting it from the plant. But of all those who have attended to this subject, no one, as far as my observation extends, has done so much as Mr. John Young, surgeon in Edinburgh. As a proof of his success, a gold medal, offered by our Horticultural Society, has been awarded to him for preparing the greatest quantity of *Lactucarium* for sale. Besides supplying others, he has sold *Lactucarium* to the Messrs. Cheynes, the acting partners of the Apothecaries' Hall, North Bridge, Edinburgh, to the extent of fifty pounds weight.

From Messrs. Cheynes practitioners may obtain not only the *Lactucarium* itself, but also the most useful preparations from it, the *Tinctura Lactucarii*, and the *Trochisci Glycyrrhizæ cum Lactucario*, carefully compounded from the article in its genuine state. And it may not be improper to observe, that practitioners have sometimes been disappointed in obtaining the effects expected

pected from this medicine, by adulterated articles having been sold under the name of *Lactucarium*, and still oftener from substitutes being employed, poppy opium being introduced where lettuce opium was prescribed, and thus exhibited to patients with whom the *Opium papaveris* always disagrees. In such cases, *Lactucarium* has sometimes been unjustly accused of being followed by disagreeable consequences.

Since the last observations which I published on the subject of *Lactucarium*, which were annexed to the third edition of my Treatise on Pulmonary Consumption, and are dated the 20th of October 1819, I have had much experience of the benefit which may be derived from it, when the genuine article is exhibited. I have not only found it highly useful in procuring sleep, in many who dread the consequences with which opium in their constitutions is attended, but also in allaying pain, in cases of tooth-ache, when externally applied to the gum. And I have particularly found it useful in allaying distressing cough, when used under the form of the *Trochisci Glycyrrhizæ cum Lactucario.*

Among other cases in which I have found great benefit from the *Lactucarium*, one occurred in my own family, and under my own roof.

One of my grandchildren, a healthy vigorous boy, in the 9th year of his age, went to school on Friday the 15th of September 1820, apparently in perfect heath. But while he dined with the family that day, he was observed to cough frequently. His cough was without expectoration, but he complained of no pain of breast; he took his food as usual, and the cough gave me no alarm. During the evening, however, he still continued to cough frequently, but did not go to bed till his usual hour of nine o'clock. He had not been in bed an hour till an elder brother, who slept in the same room with him, came down stairs to inform me that Henry's cough had increased, both in severity and frequency, and was now attended with a very peculiar noise. Before I reached his room, I had no doubt, from the peculiar and characterizing sound of the cough, that he was subjected to a very serious attack of the croup,—a disease of which I have before had more than one example in my own family. Among these, this boy's father, when nearly about his age, had been in very great danger from that disease, and to another of my sons, who was subjected to it some years after, it had proved fatal. When I reached the room in which Henry slept, his mode of respiration, and every other circumstance, afforded full confirmation of my conjecture, that he laboured under the croup.

It may readily be supposed, that I had immediate recourse to the most active measures. Blood-letting; the application of a blister; an antimonial emetic, and the inhalation of the steams

of

of warm water, were employed without delay. And I had also recourse to the use of calomel, in the manner recommended for the cure of croup, by my late worthy friend Dr. James Anderson, surgeon in Edinburgh. Under these practices his disease soon subsided; and in the space of even a few hours, my great alarm was at an end. My opinion was, that an inflammation at the head of the wind-pipe, which produced the croupy-cough, as it is called, had been cut short by the active measures employed, and that his disease might be said to be an example not of the *Cynanche Trachealis*, but of the *Cynanche Laryngea.* In the case of my son, who died of the croup about fifteen years ago, the inflammation and consequent inflammatory exudation, was not in the larynx, but entirely in the trachea: and indubitable evidence was afforded, that the affection extended not only to the under part of the trachea, but even to its large branches, the bronchiæ distributed through the lungs.

While the present case had a happy termination under the practices which I have already mentioned, it, at the same time, afforded me strong evidence of the benefit which may be derived from the *Lactucarium* lozenges, as a means of allaying a very troublesome and alarming cough. For after the most severe symptoms were removed, particularly the permanent dyspnœa, and peculiar sound in respiration, he still continued to be affected with frequent croupy-cough. For combating this, I had recourse to the *Trochisci Glycyrrhizæ cum Lactucario.* He was directed to keep one of these constantly melting in his mouth; and from the time that this practice was begun, the frequency of his cough was very much diminished, and it soon entirely left him. Thus, in the space of about twelve hours from the attack, I considered him as completely free from the disease.

I have no doubt, that if, in place of the *Lactucarium* lozenges, I had directed the ordinary opium lozenges of the *Edinburgh Pharmacopœia*, the same alleviation of cough would have been obtained; and I must candidly confess, that I have by no means the same prejudice with some of my brethren against the use of opium in inflammatory affections. I hold opium to be the most valuable medicine that has yet been discovered; and, in the words of the illustrious Sydenham, we may justly say, "sine eo, manca sit et quasi claudicet medicina." In affections decidedly of the inflammatory kind, as for example, in acute rheumatism, I am in the daily habit of employing it after blood-letting, not only without any bad effect, but with manifest benefit. If it really do possess what can be called a stimulant power, this is much more than counteracted by its sedative effects. And that it is the most powerful sedative we yet possess, is demonstrated beyond all contradiction, by its influence in allaying pain and inducing

ducing sleep. When protracted watchfulness is produced by a powerful stimulus, it is well known, that from the proper use of opium sleep may be obtained, though the stimulating cause still continues to act, as is often exemplified in tooth-ache.

But notwithstanding the high opinion I have of opium, I am still far from considering it as free from objection in every case. There are human constitutions, with which, from its peculiar narcotic power, it never can be employed with advantage. Of this I had occasion, many years ago, to witness a most deplorable example, in the case of my own father. For many years he laboured under a highly painful disease, a cancerous affection. Opium had with him, as with most others, the effect of producing a temporary alleviation of pain and some sleep. But no sooner were these consequences ended, than some of its narcotic effects, particularly sickness at stomach and vomiting, were highly distressing to him. And after trying it in every form, and in combination with every corrigent with which I am acquainted, I was obliged entirely to desert its use, though urgently required.

Cases of a similar nature are by no means rare; and in such habits, a good substitute for opium, where the alleviation of pain, or the induction of sleep, is required, is certainly of very great importance in the practice of medicine. Of all the substitutes that I have ever employed, hyoscyamus, conium, humulus, &c. I have found the *Lactucarium* or Lettuce-opium to be decidedly the best. While it possesses much of the sedative power of opium from the *Papaver*, it seems to be almost entirely free from its narcotic influence. It is, therefore, a matter of very great importance in the practice of medicine, that the shops of every apothecary should be supplied with genuine *Lactucarium*. With that view, every country apothecary should, I think, have it prepared in his own garden, which would effectually prevent all adulteration; and, according to the plan communicated by Mr. Young to the Horticultural Society, it may, with great ease, be prepared in any garden in Britain: while every apothecary in Britain, whose residence is not in a very large town, ought, in my opinion, to cultivate a garden for supplying both his kitchen and his shop. Although now far advanced in life, in the seventy-seventh year of my age, I flatter myself with the hope, that I may yet live to see *Lactucarium* find a place in the *Pharmacopœias* of other colleges, as it has already done in that of the Royal College of Physicians of Edinburgh. And that in the cure or alleviation of disease, others may obtain from it the same benefit that I have done, is the earnest wish of

A. Duncan *Sen.*

XIV. *On*

XIV. *On the North-west Magnetic Pole.* By Colonel Macdonald*.

Summerland-place, Exeter, Dec. 15.

No event will be deemed more remarkable by future ages, than the decided discovery of the *actual existence of a north-west magnetic pole*, by the hardy and enterprising navigators of the passing century. The vast importance of the fact is of the utmost consequence, as it must, infallibly, in time, lead to certain theory of the difference of the variations of the magnetic needle. Dr. Halley had recourse to the supposition of four magnetic poles belonging to a magnetic nucleus revolving within the earth, from east to west; and thus he attempted to account for the variation, and its changes, supposed to have been first observed by Columbus and Sebastian Cabot. Euler, under a very plausible and ingenious theory, supposed only two magnetic poles. Mr. Churchman adopted the idea of two magnetic poles; and imagined the northern one to move eastward, on a parallel of latitude, while the southern moved slower; the former taking 1096 years in its revolution, while the latter required 2289. The north-west pole was supposed by these philosophers to be situated not far from where the recent discovery has placed it. The deflection of the needle has been found by the navigators in the discovery ships, to have exceeded one-fourth of a great circle. Captain Cook in his voyages approached nearly, in the south hemisphere, to the supposed situations of the south magnetic poles, and found no *quantum* of variation that could at all sanction the supposition of their actuality. This certainly furnishes strong evidence that these poles are more imaginary than real; and that all future theories of variation must, necessarily, be deduced from the well-known attraction of the north and south poles of the earth, combined with the ascertained action of the north-west magnetic pole, whose positive discovery reflects so much credit on the present age. This is still further confirmed by a general remark to be made on the *variations* of Cook; viz. that in southern latitude particularly, they were, in east and west longitude, of opposite descriptions, and decidedly influenced, relatively, by the magnetic pole, whose position is now nearly known. Had the southern poles existed, the approximation to their imagined situations, by several circumnavigators, must, from the known laws of magnetism, have given rise to so strong an attraction of the south end of the magnetic needle, as would have made the variation three times, at least, greater than it has proved; independent of causing it to be of a different nature from what actually appears in the records of voyages.—Thus, then, this interesting subject seems to be reasonably cleared from the

* From The Gentleman's Magazine for December 1820.

the embarrassment of southern magnetic poles, beyond the requisite one of the south end of the earth's axis: and all future reasoning (till experience and experiment carry us further) must be founded less on hypothesis, and more on fact, than has hitherto been the case.

Having premised thus much, we come to the consideration of the wonderful and inexplicable phænomenon in nature, the accounting for which has induced so many eminent scientific characters to form the theories briefly mentioned above.—Professor Gillebrand having compared his own observations of the variation with those of others, ascertained that it gradually increased westward. In the year 1576 the variation was found to be 11° 15′ east, in London. It diminished gradually till 1662 (or 1657, by other accounts), when it became nothing; or in other words, the magnetic needle pointed to the true north. In 1666, Mr. Sellers made the variation 0° 34′ west. Since that period the variation has been increasing westward, and during the three last years it has remained nearly stationary. In comparing its progress during similar periods, it does not appear that the rate of increase is equable, as it varies from one or two minutes, to a medium annual increase of 9′ 48″.—In the Royal Society's Rooms, the mean variation in June 1817 was 24° 17′ 54″; and in June 1818 it was in mean quantity 24° 17′. In June 1819, it was found to be 24° 15′ 43″; from which it would seem that it had begun to return. It was found, by Captain Cook, that the *same* observers, with the *same* compass, in the *same* day, made a difference of five and six degrees; and nearly the double of this was found as a difference between the variation taken on the ice, and on board ship in Baffin's Bay. This leads to the clear conclusion, that observation on *terra firma* can *alone* be depended upon for *real accuracy*.

The dip of the needle, or its inclination to the nearest pole (and I have some reason to conclude that this also is subject to a daily small variation), was an accidental discovery, made by Mr. Norman, in balancing his needles. It was in 1576 found to be 71° 50′ at London; and in June 1819 it was about 70° 51′ in the Royal Society's Rooms. In the very same situation, at different periods, both the variation and dip are different; and the dip does not correspond to change in latitude under the same meridian; nor is the same dip given by the same dipping needle, at sea, on the same day. This again indicates the necessity of observing the dip *also on shore*, when real accuracy is wanted. If the variation and dip were not constantly altering in the same place, a certain theory might be arrived at; but when the contrary proves to be the fact, the attempts made to lay down on the globe a curve of no magnetic variation, is useless, if not absurd.

 A small

A small needle compared to a large real magnet, is experimentally found to furnish a similar effect to a magnetic compass-card, acted on by some invisible magnetic power within the body of the earth. This leads to the certain conclusion, that the changes in variation, and those much less in dip, arise from a corresponding change or movement in the cause. I have made the remarks contained in this paper, because I am led to believe that the recent discovery of a north-west magnetic pole has put it in the power of experimental philosophy to establish, in time, the law of movement of the magnetic cause producing effects which have hitherto baffled all human research. The anomalies of the variation observed by different persons in the same year, in London, clearly evince that the application of a needle to a meridian-line accurately laid down, can alone furnish the precise variation at the place of observation.—In two papers by me, in the Philosophical Transactions for 1796 and 1798, the process of laying off such a meridian is described; and this was done for the purpose of ascertaining accurately, not only the variation, but also the *variation of the variation*, or the diurnal vibrating variation, at Bencoolen or Sumatra; and afterwards, at the island of St. Helena. Professor Gillebrand first noticed this diurnal variation in 1635. This vibrating variation moves and returns through a few minutes of a degree, daily; and in different places its direction and quantity do not correspond. Many theories have been formed, in order to account for this extraordinary magnetic phænomenon; and the experiments made by the application of heat to magnets, afford, probably, the most plausible solution of the case: but a series of accurate observations on meridians, in many distant situations, are requisite to remove serious objections lying against the best-imagined of these conjectural theories.

From the dip of the needle it is quite unquestionable that the magnetic power, or cause, lies at some unknown depth under the surface of the earth. Mr. Æpinus, of the Imperial Academy of St. Petersburgh, has distinctly traced the close and intimate analogy between magnetism and electricity: and the Galvanic experiments lately made by the prince of chemists, Sir Humphry Davy, place the fact beyond all doubt. Experiments show that a subtle fluid of these united descriptions pervades the atmosphere, and iron, magnetised and otherwise; forming a constant and invisible communication between the magnetic cause within the earth and common magnetic needles. There is now every reason to conclude, that the *aurora borealis* constitutes a magnetic current between the real north pole and the north-west magnetic pole; the one giving out to the other an excess of fluid, in order to restore an equilibrium between positive and negative quantities. This supposition may not be quite gratuitous,

itous, as we generally observe the *aurora borealis* to act in this direction.

It is utterly impossible to attempt to account for the constant increase of the variation, without supposing that the north-west magnetic pole has a constant motion round the north pole of the earth in longitude, on a parallel of latitude; or in an elliptic curve. Though the variation, when first discovered, was only 11° 15′ east, there can be little doubt but that those who are destined to exist in the year 2040, or about that period, will find the variation as much east, as it is now west. The north pole appears to attract more powerfully than the north-west magnetic pole, as must be the case on the supposition of a revolutionary movement indispensable for the formation of any tolerable theory of the variation. The great difficulty in the way of a theory of magnetic revolution, arises from that of accounting for the fact of no variation found in some places. The solution of this difficulty may be found in a fair supposition, that the north pole, the moving magnetic pole, and the place of no variation on the surface of the earth, may be at the time nearly in one line. I found, by continued observations during two years, that the variation at Bencoolen was 1° 7′ to 1° 11′ east; the vibrating variation giving a returning swing of about four minutes of a degree. Capt. Cook found the variation in the Straits of Sunda to be 1° west.—At Condore in 8° 6′ north, and 106° 18′ east longitude, the variation was 0° 14′ west. Now these, and many other places of nearly no variation, are nearly in the line, vertical plane, or section of the two poles; and, consequently, the variation must, necessarily, be little or nothing. The well-known fact that the variation is constantly changing in one and the same place, furnishes no small proof in favour of the theory of movement of the secondary magnetic cause, or north-west magnetic pole.—The variation in London was nothing in 1662, or 158 years ago. Supposing the new pole to be situated in 100° of west longitude, it would require 568 years, nine months, and eighteen days, to effect its revolution under the parallel of its supposed movement.—In 243 years the dip of the needle appears to have diminished in London only 59 minutes of a degree. This would seem to indicate that the movement of the magnetic pole is more in a straight line, nearly in an east and west direction, than in a circular or elliptical curve, round the north pole of the earth. Bond makes the variation nothing in London in 1657. The observations regularly taken by our librarian, at the rooms of the Royal Society, may be relied on. It would appear from them, that the west variation has ceased, or turned. The variation, therefore, has taken (allowing the change to have been in 1818) about 161 years to attain its ut-

most westing. It being reasonable to suppose that the magnetic pole will move as far to the east as it has to the west of the meridian of London, the whole period of its movement in a straight line within the earth, from west to east, will be thus 322 years. In the year 1600 the variation at St. Helena was 8° east. In 1692 it was 1° west. In 1796 I make it there, from a medium of morning and evening observations on a meridian, 15° 48′ 34″·5; while in London, in 1795, it was 23° 57′.—It is experimentally found, that magnetic action, like that of heat, diminishes inversely as the squares of the distances. Again, the south pole, after passing the equator, attracts the south end of the dipping needle (which must, necessarily, possess a north polarity), and the east and west variation in south latitude are generally less than in similar situations in the northern hemisphere. The action of the south pole, combined with the other cause stated, may go far in accounting for this anomaly.

It was essentially necessary to take the foregoing view of this most wonderful and interesting subject, previously to recommending the commencement of a most important series of experiments calculated to ascertain decidedly, whether the recently discovered north-west magnetic pole has, or has not, a periodical movement corresponding to east and west variation, increasing and decreasing, as has been observed. There can be but one infallible mode of making this grand and conclusive experiment, and I take your widely circulating and valuable publication as the channel through which I earnestly call the attention of philosophers and men of science, to a sublime discovery, which British daring and fearless enterprise has, at length, put within the reach of patient and accurate investigation.

The position of the north-west magnetic pole has been approximated, to a moral certainty.—I take it for granted, that the discovery-ships will proceed again to explore to the utmost the channels in the Polar Basin, to the westward of Baffin's Bay. The principal object must be the ascertaining precisely the position of the new pole, or I would rather denominate it, the *moving magnetic power*, of whose existence no further doubt can remain.—This having been happily achieved by the bold commander, and by his companions, who have deserved so much of their country, a meridian should be accurately laid off, at some distance from the site of the new magnetic power. The graduated circumference and needle applied to this meridian, ought, in principle and construction, to resemble those used by me, and described in the papers I have alluded to. The meridian sustained by a strong post, might be sheltered by a small building devoid of iron. Careful observations made, annually, for a few years, on this meridian, would clearly determine whether or not the north-

north-west magnetic pole had a movement, and the direction and annual quantity of such movement, if thus found to take place. Huts, but no natives, have been seen in these hyperborean regions. If, however, natives should appear next year, the meridian, to remain undisturbed, might be concealed in an excavation, or situated in some secret place.

The observation of the *variation of the variation*, on this meridian, would be an important object of unremitting attention. In my papers I ascribed it to the action of the sun's heat, increased and diminished during the earth's revolution on its axis. I venture to conjecture, that this species of variation will (on the principle of heat acting on the northern poles, alternately) be found to move in an opposite direction to that observed in London. Should this prove to be the fact, the cause of the diurnal variation will be thus completely set at rest. The utmost efforts will be made to ascertain the precise position of the new pole; and if it should be impracticable to make the essential observations suggested, in its vicinity, the purpose will be equally answered by taking them to the east of Copper Mine River, at the point where west variation ceases, and east commences. The Regent's Channel may, probably, lead to this situation; if not, it can be attained to over-land, from the north-west of Hudson's Bay.

If the discovery I suggest in these imperfect statements is made in due time, it will be the greatest and most important in scientific history: and it is by giving circulation and publicity to papers of this description, that such valuable results can be arrived at.

XV. *On the Application of the Explosive Power of Coal Gas as a First Mover in Machinery.* By George Lowe, *Esq.*

To Mr. Tilloch.

Derby Brewery, Jan. 16, 1821.

Sir, — In this truly fertile age of invention, when the field of philosophic inquiry teems almost to its very highways and hedges with sportsmen, some actuated by fame, honour and pleasure; others by the stimulus of profit, in discovering "some new thing under the sun:"—To you, Mr. Editor, who see so much of these sportsmen, and who receive so much of their game, it cannot appear strange that you should so often be called upon to decide the squabbles of *anticipated shots*, and to say to whom the *feather* belongs. This is partly the reason for my now stepping out of the jog-trot path of life to present myself before you. What a truly motley group, in your eye, must this field of philosophic sportsmen present! The pen of Juvenal or the needle of Rowlandson

Rowlandson could alone delineate them to the life. Some few of them, to the honour of our country, are good shots, some bad shots, and many, like myself, random shots,—random shots be it understood in the best sense, *i. e.* those who, from the pressure of the more weighty concerns of life, can follow up their philosophic studies only at intervals, "few and far between." The game which is sought for by the first of these characters, flies high; but their keen and steady aim soon brings down their object, to the use and for the benefit of their fellow men:—such was the immortal inventor of the steam-engine—such the saver of men's lives by his safety lamp. That bad shots are to be found *long-bow* shooting in the field of philosophic sports, we need not go further a-field for proof than the recent memorable match decided in the Court of King's Bench, where we had for the first time to tax our belief, that "if boiling oil were to leak through a large leak into the fire, it would put the fire out," which certainly is an *original discovery,* though but a bad shot! But to the point for which I took pen in hand, the showing in what and by whom I find myself anticipated.

Having just read the Number of your excellent Magazine, for December, I find under the head of "Proceedings of Learned Societies," that a paper has lately been read before the Cambridge Philosophical Society, by the Rev. Dr. Cecil of Magdalen College, on the application of hydrogen gas to produce moving force in machinery. No one will more heartily congratulate the world, and Dr. Cecil, than myself, on his having perfected an engine on this principle, having had the same object in view for the last twelve months, but rather by a different means, inasmuch as I proposed (instead of hydrogen, which must be obtained at some trouble and expense) to make use of the latter portions of COAL-GAS which a retort gives off, when worked on the common routine of making gas for illumination. The gas, at this period of the operation, contains so low a per centage of carbon as to render it worse than useless to mix it along with the former portions elicited: in some gas-works it is therefore suffered to escape. Now the applying of this waste gas to some useful purpose, has been a part of my study for the improving of this truly national manufacture, and in more ways than one, I am happy to say, I have succeeded in applying it; and I doubt not, ere long, some few difficulties which at present hang over my Coal-gas exploding Engine will be wholly overcome. This project for obtaining from coal-gas a new species of moving power, I mentioned to a gentleman of high scientific attainments, and an F.R.S. residing here, as far back as last spring. The wonder is, that considering how long we have been acquainted with the tremendous powers of explosive atmospheres, their aid should

not

not have been called into action long ago. When the patent of Messrs. Gundry and Neave for a gas engine was first announced, I had little doubt but that I was then anticipated, till the publishing of their specification convinced me to the contrary.

If the above hastily written remarks should appear to you, Mr. Editor, at all conducive to the ends of science, in stimulating others who have more time and talent than the writer, their insertion in your next Number will add another favour to

Your obliged friend,

George Lowe.

XVI. *On Chemical Equivalents.* *By* Andrew Ure, *M.D. Professor of the Andersonian Institution, Glasgow.*

In compliance with our promise in our last Number, we extract this article without abridgement, from Dr. Ure's new Edition of Nicholson's Chemistry. The articles written by Dr. Ure are prefixed by an asterisk, and this article is so marked in the work. We have therefore prefixed his name to it.

* Equivalents (chemical). A term happily introduced into chemistry by Dr. Wollaston, to express the system of definite ratios, in which the corpuscular subjects of this science reciprocally combine, referred to a common standard, reckoned unity. If, with this profound philosopher, we assume oxygen as the standard, from its almost univeral relations to chemical matter, then calling it unity, we shall have, in the following examples, these ratios reduced to their lowest terms, in which the equivalents will be prime ratios:

The lowest ratio, or equivalent prime of oxygen being 1·000

That of hydrogen will be	0·125
Of fluor?	0·375
Of carbon, ...	0·750
Of phosphorus, ...	1·500
Of azote, ...	1·750
Of sulphur, ...	2·000
Of calcium, ...	2·550
Of sodium, ...	2·950
Of potassium, ...	4·950
Of copper, ...	8·00
Of barium, ...	8·75
Of lead, ...	13·00, &c.

The substances in the above table, susceptible of reciprocal saturation, can combine with oxygen or with each other, not only in

in proportions corresponding to these numbers, but also frequently in multiple or submultiple proportions. We have therefore two distinct propositions on this interesting subject.

1st, The general reciprocity of the saturating proportions.

2d, The multiple and submultiple proportions of prime equivalents, in which any one body may unite with any other body, to constitute successive binary compounds.

The first proposition, or grand law of chemical combination, was discovered by J. B. Richter of Berlin, about the year 1792. The second, of equal importance, and more recondite, was discovered so early as the year 1790, by Mr. W. Higgins.

Richter inferred his from the remarkable and well established fact, that two neutral salts, in reciprocally decomposing each other, give birth to two new saline compounds, always perfectly neutral. Thus, sulphate of soda being added to muriate of lime, will produce perfectly neutral sulphate of lime and muriate of soda. The conclusions he drew were, 1st, That the quantities of two alkaline bases, adequate to neutralize equal weights of any one acid, are proportional to the quantities of the same bases, requisite to neutralize the same weights of every other acid. For example, 6 parts of potash, or 4 of soda, neutralize 5 of sulphuric acid; and 4·4 of potash are adequate to the saturation of 5 of nitric acid. Therefore, to find the quantity of *soda* equivalent to the saturation of this weight of nitric acid, we need not make experiments, but merely compute it by the proportional rule of Richter. Thus, as 6 : 4·4 : : 4 : 2·93; or in words, as the potash equivalent to the sulphuric acid, is to the potash equivalent to the nitric acid, so is the soda equivalent to the first, to the soda equivalent to the second. And again, if 6·5 potash saturate 5 of muriatic acid gas, how much soda, by Richter's rule, will be required for the same effect? We say 6 : 6·5 : : 4 : : 4·3. 3dly, If 10·9 potash combine with 5 of carbonic acid, how much soda will be equivalent to that effect? Now, 6 : 10·9 : : 4 : 7·26. Here, therefore, we have found, that if 6 potash be equivalent to 4 soda, in saturating 5 of sulphuric acid, this ratio of 6 to 4, or 3 to 2, will pervade all the possible saline combinations; so that whatever be the quantity of potash, requisite to saturate 5, 10, &c. of any other acid, two-thirds of that quantity of soda will suffice.

In the same manner let us find out, for five of sulphuric, or of any one standard acid, the saturating quantity of ammonia, magnesia, lime, strontites, barytes, peroxide of copper, and the other bases; then their proportions to potash, thus ascertained, for this acid, will, by arithmetical reduction, give their saturating quantity of every other acid, whose relation to potash, or indeed to any one of these bases, is known.

The

The experimental verification of this most important law, occupied Richter from the year 1791 to the year 1802, in which period he published, in successive parts, a curious work, entitled The Geometry of the Chemical Elements, or Principles of Stechiometry. We might have expected greater accuracy in his investigations, from the circumstance, that Dr. Wollaston selected his statement of the constituents of nitre, in preference to those of all other chemists, in the construction of his admirable table of chemical proportions.

With indefatigable zeal Richter examined, by experiment, each acid, in its relation to the bases, and then compared the results with those given by calculation, presenting both in an extensive series of tables.

It is curious that he does not seem to have been aware, that all his tables might have been reduced into a single one, of 21 numbers, divided into two columns, by means of which, every question relating to the included articles, might be solved by the rule of three, or a sliding scale. The following table, computed by Fischer from Richter's last tables, was inserted by the celebrated Berthollet in a note to his chemical statics.

Bases.		Oxygen = 1.	Acids.		Oxygen = 1
Alumina,	525	2·625	Fluoric,	427	2·135
Magnesia,	615	3·075	Carbonic,	577	2·885
Ammonia,	672	3·36	Sebacic,	706	3·530
Lime, ..	793	3·965	Muriatic,	712	3·560
Soda, ..	859	4·245	Oxalic,	755	3·775
Strontian,	1329	6·645	Phosphoric,	979	4·895
Potash, ..	1605	8·025	Formic,	988	4·94
Barytes,	2222	1·111	Sulphuric,	1000	5·000
			Succinic,	1209	6·045
			Nitric,	1405	7·025
			Acetic,	1480	7·400
			Citric,	1683	8·415
			Tartareous,	1694	8·470

I have added the two columns under oxygen, from which we see at once, that with the exception of the bases lime, strontian, and soda, and the acids carbonic, muriatic, sulphuric, nitric, citric, and tartaric; the numbers given by Richter do not form tolerable approximations to the true proportions. The object of the above table was, to give directly the quantities of acid and alkali requisite for mutual saturation. For example, 1605, opposite to potash, is the quantity of that alkali equivalent to neu-

tralize 427 of fluoric acid, 577 carbonic, 712 muriatic, 1000 sulphuric, &c. Each column affords also progressively increasing numbers. Those nearest the top have the greatest acid or alkaline energies, as measured by their powers of saturation. The column of Richter gives, therefore, as far as the analytical means of his time permitted, a table of the relative weights of what has since been hypothetically called *the atoms.*

2. But two chemical constituents frequently unite in different proportions, forming distinct and often dissimilar compounds. Thus, oxygen and azote constitute in one proportion, nitrous oxide, the intoxicating gas of Sir H. Davy; in a second proportion, nitric oxide, the nitrous gas of Priestley; in a third proportion, nitrous acid; and in a fourth proportion, nitric acid. Is there any law regulating these various compounds; so that knowing the first proportion, we may infer the whole series? This question was first answered in a work containing many curious anticipations of discoveries, to which posterior writers have laid claim; I mean Mr. Higgins's Comparative View of the Phlogistic and Antiphlogistic Theory, printed in 1788, and published early in 1789. Besides some additional facts, decisively hostile to the hypothesis of phlogiston, this publication distinctly advances the doctrine of multiple proportions, with regard to the successive compounds of the same constituents. This was likewise interwoven, with new and ingenious views concerning gaseous and atomical combination. Mr. Higgins having felt himself aggrieved at seeing discoveries, clearly announced by him in 1789, brought forward nineteen years afterwards by Mr. Dalton, in his own name, published in 1814 a book, entitled Experiments and Observations on the Atomic Theory and Electrical Phænomena. In this work he gives numerous quotations from his *Comparative View*, which abundantly establish his claim of priority to the discovery of multiple proportions, and the atomic theory of chemistry. It is no fault of Mr. Higgins, that his first work partook of the imperfect analyses of the day. Indeed we have reason, on the contrary, to be surprised at his rejection of many errors then sanctioned by high authority, and his promulgation of many new truths, which might appear, to contemporary writers, insulated, or of little consequence, but to which subsequent researches have given a due place and importance in the system of chemical knowledge. Who would deny to Columbus the glory of discovering a new world, merely because the means of research placed within his power, did not permit him to explore its extensive coasts? Is not that glory on the contrary greatly enhanced, by the very early period at which the discovery was achieved, while navigation as a science was still unknown? I

shall

shall quote a few passages, as he gives them, from his Comparative View, which I think are decisive in this fundamental discussion.

"Hepatic gas (sulphuretted hydrogen), as shall be shown, is hydrogen in its full extent, holding sulphur in solution." This fact, of hydrogen not changing its volume, by combining with sulphur, has been marked among the valuable discoveries of later times.

"Therefore, 100 grains of sulphur require only 100 or 102 of the dry gravitating matter of oxygen gas, to form sulphurous acid. As sulphurous acid gas is very little more than double the specific gravity of oxygen gas, we may conclude, that the ultimate particles of sulphur and oxygen contain the same quantity of matter; for oxygen gas suffers no considerable diminution of its bulk, by uniting to the quantity of sulphur necessary for the formation of sulphurous acid. It contracts 1-11th, as shall be shown hereafter." Compare with the above statements the following from Dr. Thomson's System, published in 1807. "If this analysis be precise, it follows, that 100 cubic inches of hydrogen gas, in order to be converted into sulphuretted hydrogen, combine with 7·69 grains of sulphur, and are converted into about 26·6 cubic inches; so that hydrogen gas, by dissolving sulphur, is reduced to little more than one-fourth of its original bulk." Vol. i. p. 92. Sir H. Davy has since proved, by accurate experiments, that hydrogen, in its conversion into sulphuretted hydrogen, does *not* change its bulk, agreeably to Mr. Higgins's early enunciation. "But as we know the constituents of sulphuric acid, it is easy thence to deduce the following as the proportion of the ingredients of sulphurous acid:—

68 sulphur,
32 oxygen.

100." — *System of Chemistry*, 1807, vol. ii. p. 179. The last is the result of Dr. Thomson's own experiments. Its true composition is now known to be 100 of the gravitating matter of oxygen to 100 of sulphur, in conformity with Mr. Higgins.

The elementary proposition of Mr. Dalton's atomical hypothesis, seems to be most explicitly announced in the following paragraph of Mr. Higgins.

"As two cubic inches of hydrogen gas require but one cubic inch of oxygen gas to condense them to water, we may presume, that they contain an equal number of divisions, and that the difference of the specific gravity of those gases depends on the size of their respective particles; or we must suppose, that an ultimate particle of hydrogen requires two or three or more parti-

 cles

cles of oxygen to saturate it. Were this the case, water, or its constituents, might be obtained in an intermediate state of combination, like those of sulphur and oxygen, or azote and oxygen, &c. This appears to be impossible; for in whatever proportion we mix hydrogen or oxygen gases, or under whatever circumstances we unite them, the result is invariably the same. Water is formed, and the surplus of either of the gases is left behind unchanged."—"From these circumstances, we have sufficient reason to conclude, that water is composed of a single ultimate particle of oxygen, and an ultimate particle of hydrogen, and that its atoms are incapable of uniting to a third particle of either of its constituents."

Mr. Higgins inculcates very strongly, that when a body is capable of combining with another in two proportions, the third particle introduced is held by a much weaker affinity than that which unites the particles of the first or true binary compound.

"In my opinion, the most perfect nitrous acid contains 5 of oxygen and 1 of azote. Nitrous gas, according to Kirwan, contains 2 volumes of oxygen gas, and 1 of azotic gas. According to Lavoisier, 100 grains of nitrous gas contain 32 grains of azote, and 68 of oxygen. I am of the former philosopher's opinion. I also am of opinion, that every primary particle of azote is united to 2 of oxygen, and that the molecule thus formed, is surrounded with one common atmosphere of caloric.

"As this requires demonstration, let A in the annexed diagram represent an ultimate particle of azote, which attracts oxygen with the force of 3;

$$A\,\overset{3\qquad 6\qquad 3}{\rule{8em}{0.4pt}}\,a$$

Let *a* be a particle of oxygen, whose attraction to A we will suppose to be 3 more; hence they will unite with the force of 6; the nature of this compound will be hereafter explained. Let us consider this to be the utmost force of attraction that can subsist between oxygen and azote. We will now suppose a second particle of oxygen *b* to combine with A; they will only unite with the force of $4\frac{1}{2}$." "This I consider to be the real structure of a molecule of nitrous gas. Let a third particle of oxygen *c* unite to A, it will combine only with the force of 4. This is the state of the red molecules of nitrous vapour, or when condensed, the red nitrous acid." "We will suppose a fourth particle of oxygen *d* to combine with A; it will unite with the force of $3\frac{1}{4}$, and so on with the rest of the particles of oxygen as the diagram represents. This I consider to be the state of a molecule of the pale or straw-coloured nitrous acid."

"When a fifth particle of oxygen *e* unites, the force of union existing between the particles of the molecule is still diminished

as is represented by the diagram. The fractions show that the chemical attraction of azote for oxygen is nearly exhausted. This is the state of colourless nitrous acid; and, in my opinion, no more oxygen can unite to the azote, having its whole force of attraction expended in the particles *a, b, c, d, e*. This illustrates the nature of saturation or definite proportions."

"We can readily perceive from the foregoing demonstrations, that oxygen is retained with less force in the colourless nitrous acid than in the straw-coloured; and the latter acid retains it with less force than the red nitrous acid; and nitrous gas holds it with still more force than the red nitrous acid. This accounts for the separation of oxygen gas from the colourless nitrous acid (nitric acid) when exposed to the sun, at the same time that the acid becomes coloured. Nitrous acid in any other state will afford no oxygen, when exposed to the sun."

"Why the gaseous oxide should be more soluble in water than the nitrous gas, is what I cannot account for, unless it be occasioned by the smaller size of its calorific atmospheres, which may admit its atoms to come within the gravitating influence of that fluid."

It is impossible to deny the praise, of singular ingenuity, and justness, to the above passages; and every one must be struck with their analogy, both as to atomical doctrines, and the calorific atmospheres of gases, single and compound, with the language and views expanded at full length in Mr. Dalton's New System of Chemical Philosophy, first framed about the year 1803, and published in 1808. It appears that this philosopher, after meditating on the definite proportions, in which oxygen was shown by M. Proust to exist in the two oxides of the same metal, on the successive combinations of oxygen and azote, and the proportions of various other chemical compounds, was finally led to conclude, that the uniformity which obtains in corpuscular combinations, results from the circumstance, that they consist of one atom of the one constituent, united generally with one atom of the other, or with two or three atoms. And he further inferred, that the relative weights of these ultimate atoms might be ascertained from the proportion of the two constituents in a neutral compound.

Chemistry is unquestionably under great obligations to Mr. Dalton, for the pains with which he collated the various analyses of chemical bodies, by different investigators; and for establishing, in opposition to the doctrine of indefinite affinity, taught by the illustrious Berthollet, that the different compounds of the same principles did not pass into each other by imperceptible gradations, but proceeded, *per saltum*, in successive proportions,

each

each a multiple of the first. By correcting and extending Richter's scale of reciprocal saturation, and reviving Mr. Higgins's long neglected discovery of multiple proportion, Mr. Dalton has been no mean contributor to the advancement of the science. It is difficult to say how far his figured groups of spherical atoms have been beneficial or not. They may have had some use in aiding the conception of learners, and perhaps in giving a novel and imposing air to the atomical fabric. But their arrangement, and even their existence, are altogether hypothetical, and therefore ought to have no place in physical demonstrations.

That water is a compound of an atom of oxygen and an atom of hydrogen, is assumed by Mr. Dalton as the basis of his system. But two volumes of hydrogen here combine with one of oxygen. He therefore infers, that an atom of hydrogen occupies double the bulk, in its gaseous state, of an atom of oxygen. These assumptions are obviously gratuitous. I agree with Dr. Prout in thinking that Sir H. Davy has taken a more philosophical view of this subject. Guided by the strict logic of chemistry, he places no hypothesis at the foundation of his fabric.

Experiment shows, 1st, That in equal volumes oxygen weighs 16 times more than hydrogen; and 2dly, That water is formed by the union of one volume of the former, and two volumes of the latter gas, or by weight of 8 to 1. We are not in the least authorized to infer from this, that an atom of oxygen weighs 8 times as much as an atom of hydrogen. For aught we know, water may be a compound of 2 atoms of hydrogen, and 1 of oxygen; in which case we should have the proportion of the weights of the atoms, as given by equal volumes, namely, 1 to 16. There is no good reason for fixing on one compound of hydrogen, more than on another, in the determination of the basis of the equivalent scale. If we deliberate on that combination of hydrogen in which its agency is apparently most energetic, namely, that with chlorine, we should surely never think of pitching on *two* volumes as its *unity* or least proportion of combination; for it is *one* volume of hydrogen which unites with one volume of chlorine, producing two volumes of muriatic gas. Here, therefore, we see that *one* volume of hydrogen is quite adequate to effect, in an active gaseous body of equal bulk, and 36 times its weight, an entire change of properties. Should we assume in gaseous chemistry, 2 volumes of hydrogen, as the combining unit, or as representing an atom; then it should never unite in 3 volumes, or an atom and a half with another gas. Ammonia, however, is a compound of 3 volumes of hydrogen with 1 of azote; and if 2 volumes of hydrogen to 1 of oxygen be called an atom to an atom, surely 3 volumes of hydrogen to 1 of azote should be called

called an atom and an half to an atom. Yet the Daltonian commentator, on the second occasion, counts one volume an atom of hydrogen, and on the first, two volumes an atom.

We should steer clear of all these gratuitous assumptions and contradictions, by making a single volume of hydrogen represent its atom, or prime equivalent. "There is an advantage," says Dr. Prout, "in considering the volume of hydrogen equal to the atom, as in this case, the specific gravities of most, or perhaps all elementary substances (hydrogen being one) will either exactly coincide with, or be some multiple of, the weights of their atoms; whereas, if we make the volume of oxygen unity, the weights of the atoms of most elementary substances, except oxygen, will be double that of their specific gravities, with respect to hydrogen. The assumption of the volume of hydrogen being equal to the atom, will also enable us to find more readily, the specific gravities of bodies in their gaseous state, (either with respect to hydrogen, or atmospheric air,) by means of Dr. Wollaston's logometric scale.

"If the views we have ventured to advance be correct, we may almost consider the πρωτη ὕλη of the ancients to be realized in hydrogen; an opinion, by the by, not altogether new. If we actually consider this to be the case, and futher consider the specific gravities of bodies, in their gaseous state, to represent the number of volumes condensed into one; or in other words, the number of the absolute weight of a single volume of the first matter (πρωτη ὕλη) which they contain, which is extremely probable; multiples in weight must always indicate multiples in volume, and *vice versâ*; and the specific gravities or absolute weights of all bodies in a gaseous state, must be multiples of the specific gravity, or absolute weight of the first matter, (πρωτη ὕλη), because all bodies in a gaseous state, which unite with one another, unite with reference to their volume."

From these ingenious observations, we perceive the singular felicity of judgement, with which Sir H. Davy made choice of the single volume of hydrogen, for the unit of primary combination, in his Elements of Chemical Philosophy.

Mr. Dalton's prelections on the atomic theory, and Dr. Thomson's commentary, had excited but a feeble sensation in the chemical world. That part of his system which treated on caloric, was blended with so much mere hypothesis, that chemists transferred a portion of the scepticism thus created, to his collation of primary and multiple combinations. It was Dr. Wollaston who first decided public opinion in favour of the doctrine of multiple proportions, by his elegant paper on super-acid and sub-acid salts, inserted in the Philosophical Transactions for 1808.

1808. The object of the atomic theory has been no where so happily stated as by this philosopher, in the following sentence:

"But, since the publication of Mr. Dalton's theory of chemical combination, as explained and illustrated by Dr. Thomson, (System, 3d edit.) the inquiry which I had designed appears superfluous, as all the facts I had observed are but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms."

It is evident from this passage, that the principle which presented itself to Mr. Dalton, on a review of the labours of *other* chemists, had really occurred to Dr. Wollaston from his *own*, and that he would unquestionably have been speedily led to its full development. If Mr. Dalton had ever chanced to look into the neglected book of Higgins, there would have been little merit in his anticipation of what the advancement of analytical precision would infallibly have revealed in a very short period.

Dr. Wollaston, in the above decisive paper, demonstrates, that in the sub-carbonate and crystallized carbonate of potash, the relation of the carbonic acid to the base, in the first is exactly one-half of what it is in the second. The same law is shown to hold with regard to the two carbonates of soda, and the two sulphates of potash; and being applied to his experiments on the compounds of potash and oxalic acid, leads him to conclude, that the neutral oxalate may be considered as consisting of 2 particles potash, 1 acid; the binoxalate as 1 and 1, or 2 potash, with 2 acid; the quadroxalate as 1 and 2, or 2 potash, with 4 acid.

We cannot withhold from our readers the following masterly observations, which must make every one regret, that the first development of the atomic theory had not fallen into such philosophical hands.

"But an explanation which admits a double share of potash in the neutral salts (the oxalates) is not altogether satisfactory; and I am further inclined to think, that when our views are sufficiently extended, to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement, in all the three dimensions of solid extension.

"For instance, suppose the limit to the approach of particles, to be the same in all directions, and hence their virtual extent

to

to be spherical (which is the most simple hypothesis); in this case, when different sorts combine singly, there is but one mode of union. If they unite in the proportion of two to one, the two particles will naturally arrange themselves at opposite poles of that to which they unite. If they be three, they might be arranged with regularity at the angles of an equilateral triangle, in a great circle surrounding the single spherule; but in this arrangement, for want of similar matter at the poles of this circle, the equilibrium would be unstable, and would be liable to be deranged by the slightest force of adjacent combinations: but, when the number of one set of particles exceeds in the proportion of 4 to 1, then, on the contrary, a stable equilibrium may again take place, if the four particles are situated at the angles of the four equilateral triangles composing a regular tetrahedron.

"But as this geometrical arrangement of the primary elements of matter is altogether conjectural, and must rely for its confirmation or rejection upon future inquiry, I am desirous that it should not be confounded with the results of the facts and observations related above, which are sufficiently distinct and satisfactory with respect to the existence of the law of simple multiples. It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known; since, even admitting that a very small number of these atoms combining together, would have a tendency to arrange themselves in the manner I have imagined; yet until it is ascertained how small a proportion the primary particles themselves bear to the interval between them, it may be supposed that surrounding combinations, although themselves analogous, might disturb this arrangement; and in that case, the effect of such interference must also be taken into the account, before any theory of chemical combination can be rendered complete."

I am not aware, that any chemist has adduced experimental evidence, to prove that a "stable equilibrium may again take place, if the four particles are situated at the angles of the four equilateral triangles composing a regular tetrahedron." I have, therefore, much pleasure in referring to my researches on the constitution of liquid nitric acid, as unfolding a striking confirmation of Dr. Wollaston's true philosophy of atomical combination. When I wrote the following sentence, I had no recollection whatever of Dr. Wollaston's profound speculations on tetrahedral arrangement.—"We perceive, that the liquid acid of 1·420, composed of 4 primes of water + 1 of dry acid, possesses the greatest power of resisting the influence of temperature to change its state. It requires the *maximum* heat to boil it, when it distills unchanged; and the maximum cold to effect its congelation." See ACID (NITRIC), in this Dictionary.

Here we have a fine example of the stability of equilibrium, introduced by the combination of four atoms with one. The discovery which I had also the good fortune to make with regard to the constitution of aqueous sulphuric acid, that the maximum condensation occurred, when one atom of the real acid was combined with three atoms of water, is equally consonant to Dr. Wollaston's views. "But in this arrangement," says Dr. Wollaston, "for want of similar matter at the poles of this circle, the equilibrium would be unstable, and would be liable to be deranged by the slightest force of adjacent combinations." Compare with this remark, the following sentence from my paper on sulphuric acid, as published in the Journal of Science, October 1817.—"The terms of dilution are, like logarithms, a series of numbers in arithmetical progression, corresponding to another series, namely, the specific gravities, in geometrical progression. For a little distance on both sides of the point of greatest condensation, the series converges with accelerated velocity, whence the 10 or 12 terms on either hand, deviate a little from experiment." Page 126. Or in other words, a small addition of water or of acid to the above atomic group, produces a *great* change on the degree of condensation; which accords with the position "that the equilibrium would be liable to be deranged by the slightest force of adjacent combinations."

While considering this part of Dr. Wollaston's important paper, let me advert to the curious facts pointed out in the article Nitric Acid, relative to the compound of one atom of dry acid and seven atoms water. In my paper on the subject, published in the eighth number of the Journal of Science, I showed, that this liquid combination was accompanied with the greatest condensation of volume, and the greatest disengagement of heat. In composing this Dictionary, I calculated, for the first time, the atomical constitution of the nitric acids employed by Mr. Cavendish, for congelation; and found with great satisfaction, that the same proportion which had exhibited, in my experiments, the most intense reciprocal action, as was indicated both by the aggregation of particles, and production of heat, was likewise that which most favoured solidification. Such acid congeals at $-2°$; but when either stronger or weaker, it requires a much lower temperature for that effect.

3. The next capital discovery in multiple proportions, was made by M. Gay-Lussac, in 1808, and published by him in the second volume of the *Memoires d'Arcueil.* After detailing a series of fine experiments, he deduces the following important inferences:—"Thus it evidently appears, that all gases, in their mutual action, uniformly combine in the most simple proportions; and we have seen, in fact, in all the preceding examples, that

that the ratio of their union is that of 1 to 1, of 1 to 2, or of 1 to 3, by volume. It is important to observe, that when we consider the weights, there is no simple and definite relation between the elements of a first combination; it is only when there is a second between these same elements, that the new proportion of that body which has been added, is a multiple of the first. Gases, on the contrary, in such proportions as can combine, give rise always to compounds whose elements are in volume multiples the one of the other.

"Not only do the gases combine in very simple proportions, as we have just seen, but moreover the apparent contraction of volume which they experience by combination, has likewise a simple relation with the volume of the gases, or rather with the volume of one of them."

By supposing the contraction of volume of the two gaseous constituents of water to be only equal to the whole volume of oxygen added, he found the ratio of the density of steam to be to that of air as 10 to 16; a computed result in exact correspondence with the experimental result lately obtained in an independent method, by the same excellent philosopher. "Ammoniacal gas is composed in volume," says he, "of 3 parts of hydrogen and 1 of azote, and its density, compared to that of air, is 0·596; but if we suppose the apparent contraction to be one-half of the total volume, we find 0·591 for its density. Thus it is demonstrated by this nearly perfect accordance, that the apparent contraction of its elements is precisely one-half of the total volume or rather double the volume of azote." M. Gay-Lussac subjoins to his beautiful memoir a table of gaseous combination, which, with some modifications derived from subsequent researches, will be inserted under the article GAS.

The same volume of the *Memoires* presents another important discovery of M. Gay-Lussac, on the subject of equivalent proportions. It is entitled, On the relation which exists between the oxidation of metals, and their capacity of saturation for the acids. He here proves by a series of experiments, that the quantity of acid which the different metallic oxides require for saturation, is in the direct ratio of the quantity of oxygen which they respectively contain. "I have arrived at this principle," says he, "not by the comparison of the known proportions of the metallic salts, which are in general too inexact to enable us to recognise this law, but by observing the mutual precipitation of the metals, from their solutions in acids."

When we precipitate a solution of acetate of lead, by a plate of zinc, there is formed a beautiful vegetation known under the name of the *tree of Saturn*; and which arises from the reduc-

tion of the lead by a galvanic process, as was first shown by Silvester and Grotthus. We obtain at the same time a solution of acetate of zinc, equally neutral with that of the lead, and entirely exempt from this last metal. No hydrogen, or almost none, is disengaged during the precipitation; which proves, that the whole oxygen necessary to the zinc, for its becoming dissolved and saturating the acid, has been furnished to it by the lead.

If we put into a solution of sulphate of copper, slightly acidulous, bright iron turnings in excess, the copper is almost instantly precipitated; the temperature rises, and no gas is disengaged. The sulphate of iron which we obtain, is that in which the oxide is at a minimum, and its acidity is exactly the same as that of the sulphate of copper employed.

We obtain similar results, by decomposing the acetate of copper by lead, especially with the aid of heat. But since the zinc precipitates the lead from its acetic solution, we may conclude, that it would also precipitate copper, from its combination with the acetic acid. Experience is here in perfect accordance with theory.

We know with what facility copper precipitates silver, from its nitric solution. All the oxygen which it needs for its solution, is furnished to it by the oxide of silver; for no gas is disengaged, and the acidity is unchanged. The same thing happens with copper, in regard to nitrate of mercury, and to cobalt, in regard to nitrate of silver. In these last examples, as in the preceding, the precipitating metal finds in the oxide of the metal, which it precipitates, all the oxygen which is necessary to it for its oxidizement, and for neutralizing to the same degree the acid of the solution.

These incontestable facts naturally conduct to the principle announced above, that the acid in the metallic salts is directly proportional to the oxygen in their oxides. In the precipitation of one metal by another, the quantity of oxygen in each oxide remains the same, and consequently the larger dose of oxygen the precipitating metal takes, the less metal will it precipitate.

M. Gay-Lussac next proceeds to show, with regard to the same metals at their different stages of oxidizement, that they require of acid, a quantity precisely proportional to the quantity of oxygen they may contain; or that the acid in the salts, is exactly proportional to the oxygen of the oxides. A very important result of this law, is the ready means it affords of determining the proportions of all the metallic salts. The proportions of one metallic salt, and the oxidation of the metals, being given, we may determine those of all the salts of the same genus; or the proportions of acid, and of oxide, of all the metallic salts, and the oxidation of a single metal being given, we can calculate the oxidation

oxidation of all the rest. Since the peroxides require most acid, we can easily understand how the salts containing them, should be in general more soluble than those with the protoxide.

M. Gay-Lussac concludes his memoir with this observation: When we precipitate a metallic solution, by sulphuretted hydrogen, either alone or combined with an alkaline base, we obtain a sulphuret or a metallic hydrosulphuret. In the first case, the hydrogen of the sulphuretted hydrogen combines with all the oxygen of the oxide, and the sulphur forms a sulphuret with the metal; in the second case, the sulphuretted hydrogen combines directly with the oxide, without being decomposed, and its proportion is such, that there is sufficient hydrogen to saturate all the oxygen of the oxide. The quantity of hydrogen neutralized, or capable of being so, depends therefore on the oxidation of the metal, as well as the quantity of the sulphur which can combine with it. Of consequence, the same metal forms as many distinct sulphurets, as it is susceptible of distinct stages of oxidation in its acid solutions. And as these degrees of oxidation are fixed, we may also obtain sulphurets, of definite proportions, which we can easily determine, according to the quantity of oxygen to each metal, and the proportions of sulphuretted hydrogen.

The next chemist who contributed essentially to the improvement of the equivalent ratios of chemical bodies, was Berzelius. By an astonishing number of analyses, executed for the most part with remarkable precision, he enabled chemical philosophers to fix with corresponding accuracy, the equivalent ratios reduced to their lowest terms. He himself took oxygen as the unit of proportion.

The results of all this emulous cultivation, were combined and illustrated with original researches, by Sir H. Davy, in his Elements of Chemical Philosophy published in 1812. In this system of truths, which will never become obsolete, we find the claims of Mr. Higgins to the discovery of the atomic theory justly advocated.

But what peculiarly characterizes this chemical work, is the sound antihypothetical doctrines which it inculcates on chemical combination. "Mr. Higgins," says Sir H., "has supposed that water is composed of one particle of oxygen and one of hydrogen, and Mr. Dalton of an atom each; but in the doctrine of proportions derived from facts, it is not necessary to consider the combining bodies, either as composed of indivisible particles, or even as always united, one and one, or one and two, or one and three proportions. Cases will be hereafter pointed out, in which the ratios are very different; and at present, as we have no means whatever of judging either of the relative numbers, figures,

figures, or weights of those particles of bodies which are not in contact, our numerical expressions ought to relate only to the results of experiments."

He conceives that the calculations will be much expedited, and the formulæ rendered more simple, by considering the smallest proportion of any combining body, namely, that of hydrogen, as the integer. This radical proportion of hydrogen is the πρωτη ὕλη of the ancient philosophers.

It has been objected by some, to our assuming hydrogen as the unit, that the numbers representing the metals would become inconveniently large. But this could never be urged by any person acquainted with the theory of numbers. For in what respect is it more convenient to reckon barium 8·75 on the atomic scale, or $8{\cdot}75 \times 16 = 140$ on Sir H. Davy's scale of experiment? or is it any advantage to name, with Dr. Thomson, tin $= 7{\cdot}375$, or to call it 118, on the plan of the English philosopher? If the combining ratios of all bodies be multiples of hydrogen, as is probable, why not take hydrogen as the unit? I think this question will not be answered in the negative, by those who practise the reduction of chemical proportions. The defenders of the Daltonian hypothesis, that water consists of one atom oxygen to one atom hydrogen, may refer to Dr. Wollaston's scale, as authority for taking oxygen as the unit. But that admirable instrument, which has at once subjected thousands of chemical combinations to all the dispatch and precision of logometric calculation, is actually better adapted to the hydrogen unit, than to the oxygen. For if we slide down the middle rule, till 10 on *it* stand opposite to 10 hydrogen on the left side, every thing on the scale is given in accordance with Sir H. Davy's system of primary proportions, and M. Gay-Lussac's theory of gaseous combination. This valuable concurrence, as is well pointed out by Dr. Prout, we lose, by adopting the volume of oxygen as *radix*.

In the first part of the Phil. Transactions for 1814 appeared Dr. Wollaston's description of his scale of chemical equivalents, an instrument which has contributed more to facilitate the general study and practice of chemistry than any other invention of man. His paper is further valuable, in presenting a series of numbers denoting the relative primary proportions, or weights of the atoms of the principal chemical bodies, both simple and compound, determined with singular sagacity, from a general review of the most exact analyses of other chemists, as well as his own.

The list of substances which he has estimated, are arranged on one or other side of a scale of numbers, in the order of their relative weights, and at such distances from each other, according

to

to their weights, that the series of numbers placed on a sliding scale can at pleasure be moved, so that any number expressing the weight of a compound, may be brought to correspond with the place of that compound, in the adjacent column. The arrangement is then such, that the weight of any ingredient in its composition, of any reagent to be employed, or precipitate that might be obtained in its analysis, will be found opposite the point at which its respective name is placed.

If the slider be drawn upwards, till 100 corresponds to muriate of soda, the scale will then show how much of each substance contained in the table, is equivalent to 100 of common salt. It shows, with regard to the different views of this salt, that it contains 46·6 dry muriatic acid, and 53·4 of soda, or 39·8 sodium, and 13·6 oxygen; or if viewed as chloride of sodium, that it contains 60·2 chlorine, and 39·8 sodium. With respect to reagents, it may be seen, that 283 nitrate of lead, containing 191 of litharge employed to separate the muriatic acid, would yield a precipitate of 237 muriate of lead, and that there would then remain in solution, nearly 146 nitrate of soda. It may at the same time be seen, that the acid in this quantity of salt would serve to make 232 corrosive sublimate, containing 185·5 red oxide of mercury; or make 91·5 muriate of ammonia, composed of 62 muriatic gas (or hydromuriatic acid), and 29·5 ammonia. The scale shows also, that for the purpose of obtaining the whole of the acid in distillation, the quantity of oil of vitriol required is nearly 84, and that the residuum of this distillation would be 122 dry sulphate of soda, from which might be obtained, by crystallization, 277 of Glauber salt, containing 155 water of crystallization. These, and many more such answers, appear at once, by bare inspection, as soon as the weight of any substance intended for examination is made, by motion of the slider, correctly to correspond with its place in the adjacent column. Now surely the accurate and immediate solution of so many important practical problems, is an incalculable benefit conferred on the chemist.

With regard to the method of laying down the divisions of this scale, those who are accustomed to the use of other sliding rules, and are practically acquainted with their properties, will recognise upon the slider itself, the common Gunter's line of numbers, (as it is called) and will be satisfied, that the results which it gives are the same that would be obtained by arithmetical computation.

Those who are acquainted with the doctrine of ratios, and with the use of logarithms as measures of ratios, will understand the principle on which this scale is founded. and will not need to be told, that all the divisions are logometric; consequently, that the

the mechanical addition and subtraction of ratios here performed by juxtaposition, correspond in effect to the multiplication and division of the numbers, by which those ratios are expressed in common arithmetical notation.

In his Essay on the Cause of Chemical Proportions, Berzelius proposed a system of signs, to denote atomical combinations, which it may be proper briefly to explain. This sign is the initial letter, and by itself always expresses one atom, volume, or prime of the substance. When it is necessary to indicate several volumes or primes, it is done by prefixing the number. For example, the cuprous oxide, or protoxide of copper, is composed of a prime of oxygen and a prime of metal; its sign is therefore $Cu+O$. The cupric oxide, or deutoxide of copper, is composed of 1 prime metal, and 2 primes oxygen; therefore its sign is $Cu+2O$. In like manner, the sign for sulphuric acid is $S+3O$; for carbonic acid, $C+2O$; for water, $2H+O$, &c.

When we express a compound prime of the first order, or binary, we throw away the +, and place the number of primes above the letter, as the index or exponent is placed in arithmetic. For example, $CuO+SO^3 =$ sulphate of copper; $CuO^2+2SO^3 =$ bi-deutosulphate of copper, or persulphate. These formulæ have this advantage, that if we take away the oxygen, we see at once the ratio between the radicals. As to the primes of the second order, or ternary compounds, it is but rarely of any advantage to express them by formulæ, as one prime; but if we wish to express them in that way, we may do it by using the parenthesis, as is done in algebraic formulæ: for example, according to Berzelius, alum is composed of 3 primes of sulphate of alumina, and 1 of sulphate of potash. Its symbol is $3(Al\,O^2+2SO^3)+(Po^2+2SO^3)$. The prime of ammonia is $3HN$; viz. 3 primes hydrogen + 1 nitrogen. We shall use these abbreviations in our table of equivalent primes, at the end of the volume.

To reduce analytical results, as usually given for 100 parts, to the equivalent prime ratios, or, in hypothetical language, to the atomic proportions, is now a problem of perpetual recurrence, with which students are perplexed, as no rule has been given for its ready solution. Though numerous examples of its solution occur in this Dictionary, we shall here explain it in detail.

As in all reasoning we must proceed from what is known or determinate, to what is unknown or indeterminate, so in every analysis, there must be one ingredient whose prime equivalent is well ascertained. This is employed as the common measure, and the proportions of the rest are compared to it. Let us take, for instance, Sir H. Davy's analysis of fluate of lime, to determine the unknown number, that should denote the prime of fluoric acid. We know, first of all, that 2 primes of oxygen $= 2$,

combine

combine with 1 of carbon =0·75, to form the compound prime 2·75 of carbonic acid. We likewise know that carbonate of lime consists of 43·6 carbonic acid + 54·4 lime. We therefore make this proportion, to determine the prime equivalent of lime.

1. 43·6 : 54·4 : : 2·75 : 356= prime of lime.

2. We know that 100 parts of dry sulphate of lime consist of 41·6 lime and 58·4 acid. Hence, to find the prime of sulphuric acid, we make this proportion:

41·6 : 58·4 : : 3·56 : 5 = prime of sulphuric acid.

3. Sir H. Davy obtained from 100 grains of fluor spar in powder, acted on with repeated quantities of sulphuric acid, and ignited, 175·2 grains of sulphate of lime. Now, since 100 grains of sulphate of lime contain, as above, 41·6 of lime, we have this proportion:

100 : 41·6 : : 175·2 : 72·88 = lime, correponding to 175·2 grains of sulphate, and which previously existed in the 100 grs. of fluor spar. If from 100 we subtract 72·88, the difference 27·12 is the fluoric acid, or the other ingredient of the fluor, which saturated the lime. Now to find its prime equivalent, we say,

72·88 : 3·56 : : 27·12 : 1·325 = the prime or atom of fluoric acid from Sir H. Davy's experiment. Had we taken Dr. Thomson's number 3·625, as representing the atom of lime, the atom of fluoric acid would come out 1·3015. As the Doctor had a particular hypothesis to support, which required the weight of the acid atom to be a great deal less, he deduces it, from the same *data*, to be only 1·0095. By what new process of arithmetic he brought out this number, it is impossible to conjecture. But no doubt he devoted some pains to the computation, since he rears on that unsubstantial basis, a long fabric of atomic induction.

We shall give another example, derived from a more complex subject.

M. Vauquelin found, that 33 parts of lime, saturated with sorbic acid, and carefully dried, weighed 100 grains. Hence the difference, 67 grains, was acid. To find its equivalent prime, we say,

As 33 : : 67 : : 3·56 = the prime of lime : 7·23 = the prime of the acid. But as he brought it to absolute neutrality by a small portion of potash, we may take 7·5 for the prime.

M. Vauquelin subjected the acid as it exists in the dry sorbates of lead and copper, to igneous analysis; and obtained the following results:

Hydrogen	16·8
Carbon	28·3
Oxygen	54·9
	100·0

 Now

Now we must find such an assemblage of the primes or atoms of these elements, as will form a sum-total of 7·5; and at the same time be to each other, in the above proportions. The following very simple rule will give a ready approximation; and by a common sliding scale, it may be worked by inspection.

Multiply each proportion per cent. by the compound prime, and compare the products with the multiples of the constituent primes. You can then estimate the number of each prime requisite to compose the whole. Thus,

			Theory.	Exp.
0·168 × 7·5 = 1·2600 or	10 hydrogen	= [illegible]5	16·7	16·8
0·283 × 7·5 = 2·1225	3 carbon	= 2·25	30·0	28·3
0·549 × 7·5 = 4·1175	4 oxygen	= 4·00	53·3	54·9
		7·50	100·0	100·0

The differences between these theoretical and experimental proportions, are probably within the limits of the errors of the latter, in the present state of analysis.

If on Dr. Wollaston's scale, we mark with a type or a pen, 2h, 3h, &c. up to 10h; 2c, 3c, 4c, 5c; and 2n, 3n, 4n; respectively opposite to twice, thrice, &c. the atoms of hydrogen, carbon, and nitrogen, as is already done for oxygen (with the exception of the fourth, where copper stands) we shall then have ready approximations to the prime components, by inspection of the scale. Move the sliding part, so that one of the quantities per cent. may stand opposite the nearest estimate of a multiple prime of that constituent. Thus we know that hydrogen, carbon, and oxygen, bear the relation to each other of 1, 6, 8; and, of course, the latter two, that of 3 to 4. But 54·9 oxygen, being more than one-half of 100, the weight of oxygen in the compound prime is more than the half of 7·5, and therefore points to 4. Place 54·9 opposite 4 oxygen (where copper stands) we shall find 18 opposite 10 hydrogen, and 30·7 opposite 3 carbon. Here we see the proportions of carbon and hydrogen, are both greater than by Vauquelin's analysis. Try 51 opposite 4 oxygen, then opposite 3 carbon we have, 28·7, and opposite 10 hydrogen 16·9. The proportions I have calculated arithmetically above, seem somewhat better approximations; they were deduced from hydrogen 0·125, and carbon 0·75, instead 0·132 and 0·754, as on the scale.

If the weight of the compound prime is not given, then we must proceed to estimate the nearest prime proportions, after inspection of those per cent. The scale may be used with advantage, as just now explained.

The following case has been reckoned difficult of solution, and has been even involved in an algebraic formula. Let us suppose

a vegetable acid, containing combined water, whose prime equivalent is to be determined by experiment. A crystallized salt is made with *it*, for example, and a determinate quantity of soda. Suppose the alkali to form 26 per cent. of the salt. The rest is water and acid. Dissolve 100 grains, and add them to an indefinite quantity of the solution of any salt, with whose base the vegetable acid forms an insoluble compound. Dry and weigh this precipitate. Without decomposing the latter, we have sufficient data for determining the prime equivalent of the real acid. We make this proportion: As the weight of soda is, to *its* prime equivalent, so is the weight of the precipitate to the prime of the compound. Suppose 148 grains of an insoluble salt of lead to have been obtained; then 26 : 3·95 : : 148 : 22·1 = the prime of the salt of lead. From this, if we deduct the weight of the prime equivalent of oxide of lead, =14, we have 8·1 for the prime equivalent of the acid. And the crystallized salt must have consisted of

Dry acid,	..	53·3
Soda, ..	..	26·0
Water,	..	20·7
		100·0

As the above numbers were assumed merely for arithmetical illustration, the water is not atomically expressed. Indeed the problem of finding the acid prime, does not require the salt to be either dried or weighed. A solution would suffice. Saturate a known weight of alkali, with an unknown quantity of the crystallized acid. Add this neutral solution, to a redundant quantity of solution of nitrate of lead. Wash, dry, and weigh the insoluble precipitate, and apply the above rule.

There are three systems of equivalent numbers at present employed: 1st, That having oxygen as the radix; 2d, That having one volume of hydrogen as the radix; 3d, That having two volumes of hydrogen as the radix, on the Daltonian supposition, that two volumes of hydrogen contain the same number of atoms, as one volume of oxygen. As this hypothesis is destitute of proof, it evidently should be discarded from physical science. Since the volume of hydrogen, is equal in weight to 1-16th the weight of the volume of oxygen, the former two systems are mutually convertible, by multiplying the number oxygen, in the oxygen ratio, by 16, or 4 × 4, to obtain the number in the hydrogen scale; and this is reconverted by the inverse operation, namely, dividing by 16, or by 4 × 4.

Dr. Wollaston's scale, and Sir H. Davy's proportional numbers, are adapted to the idea that water is a compound of 1 hydrogen + 7·5 oxygen by weight, or 15 + 1 by volume.

 Their

Their mutual conversion is therefore very easy, for if we add to Dr. Wollaston's number, its half, the sum is Sir H. Davy's; and of course, if we subtract from the number of the latter, its third, the remainder is Dr. Wollaston's number. There is one very frequent variation in the weights of the primes among the best writers, namely, doubling or halving the number. This difference is occasioned generally by an uncertainty about the first term or proportion in which the body combines with oxygen; some chemists reckoning that a protoxide [illegible] others consider a deutoxide. Thus Sir H. Davy gives 10[illegible] the number representing iron; from which, if we deduct $\frac{[illegible]}{3} = 34{\cdot}3$, the remainder 68·7 is nearly double of 34·5, the number of Dr. Wollaston. But Mr. Porrett has very ingeniously shown that perhaps $\frac{35}{2} = 17{\cdot}5$, is to be preferred.*

XVII. *Dr.* Granville's *Reply to a Review in Professor* Brande's *Journal of Science.*

The accompanying reply to an article contained in Mr. Brande's Journal for January 1821, on the subject of my work on Prussic Acid, was sent to that gentleman, through the hands of a friend, for insertion in his next number. The result of this application will be better understood by a perusal of the following laconic correspondence.

Saville-Row, Wednesday Evening, Feb. 7.

"Dear Sir,—I send you a reply to the article contained in your last Number, purporting to be a review of my work on the Prussic Acid. Dr. Hutchinson, a particular friend of mine, has been kind enough to undertake to deliver it into your own hands, to prevent mistakes; and he is instructed to request you will have the goodness to name an hour in the evening of to-morrow, when he may call for your decision, as to whether you will admit, or not, the said reply for insertion in your next number for April. In case of your declining to insert it—a circumstance which I am far from contemplating—I have requested my friend to bring back the manuscript, without any further comment.

"I have the honour to be, your humble servant,

"*W. T. Brande, Esq. F.R.S. &c.* A. B. Granville."

Two days afterwards, Mr. Brande returned me the manuscript, with a short note, of which the following is the beginning:

Thursday

Thursday (not delivered till Friday night).

"My dear Sir,—You must surely be quizzing me, to suppose that I should insert the inclosed, &c.

"I am always yours, faithfully,

"*Dr. Granville, Saville-row.* W. T. Brande."

How far, by the refusal of an act of justice and impartiality demanded of him, and by the language in which that refusal is conveyed, Mr. Brande has or has not identified himself with my reviewer—and thereby rendered himself obnoxious to all and each of the charges I have brought forward and, I hope, *proved* against the latter,—I leave the reader to decide. It is enough for me to observe that Mr. Brande's conduct, as the editor of the Quarterly Journal, in this affair, is to me a matter of great astonishment; and that, as a member of the Royal Institution, I shall take the earliest opportunity of protesting, either at a general meeting, or to the board of managers, against that Society's lending its name to a journal in which an attack, involving matters of personal consideration, is admitted, and the reply, showing the injustice and unfairness of that attack, rejected.

Under these circumstances I avail myself of the facility, which the Philosophical Magazine liberally affords me, of giving publicity to my reply to the review in question.

Saville Row, A. B. Granville.
Friday, 9th February, 1821.

A Reply to an Article inserted in the 20th Number of the Quarterly Journal of Science, edited at the Royal Institution—purporting to be a Review of Dr. Granville's *Treatise on the internal Use of the Hydrocyanic Acid. In a Letter to* W. T. Brande, *Esq. F.R.S. the Editor.*

Dear sir,—You will readily remember, that when you informed me of having received a "severe" critique on my work on Prussic Acid for insertion in your Journal, of which, however, you assured me that no use would be made, if I no longer entertained the opinion I expressed in that work respecting the acid prepared at Apothecaries' Hall—I instantly replied, that you were welcome to admit and insert the article in question, if you thought proper: for as my opinion of the acid prepared at the Hall, at the specific period at which I was writing, had been formed upon such ocular and experimental demonstration as would have warranted still stronger expressions of disapprobation on my part—I could not tamely surrender my humble judgement to the terror of any review of my book, however severe. The only concession I claimed in return was, that any reply that I might think it necessary to write to the article you mentioned, should

should be equally honoured with insertion in your Journal—and to the justice of this claim you readily acceded*.

In order to bring these circumstances more particularly to your memory, it will be well to mention that the conversation above referred to took place on the evening of the 14th of December 1820, at Somerset House, a short time before the meeting of the Royal Society; and that it was again repeated by you, in a more cursory, yet impressive manner, immediately after the Society had adjourned, as we were both in the act of leaving the meeting room.

Had the review been "severe," yet just, correct, and candid, I should have preserved a becoming silence, and endeavoured to profit by the admonitions bestowed on me, however harshly; but as it possesses none of the latter qualities, I can scarcely be expected to suffer it to pass unnoticed.

When I state that the review is the reverse of being just, correct, and candid, I am advancing what, I trust, I shall fully prove to yourself and the public; and this circumstance induces me to declare, *in limine*, that, although by designating that review as "severe," you implicitly acknowledged that you had read it; yet, from my acquaintance with your character and urbanity of manners, I am free to assume that you had not paid any very particular attention to the structure of that article, and the assertions it comprises; or it would never have appeared in your Journal, to which I am proud in having been one of the earliest, and certainly not the least zealous of its contributors.

My observations, therefore, can, in no way whatever, apply to you as the Editor of that Journal, except where it is explicitly so stated; but are directed to the writer, of whom the letter O stands as the representative.

To render my reply as perspicuous and as concise as the nature of the subject will admit, I shall, whenever I have an opportunity, adopt the form of *positive answer* to the assertions of the reviewer—rather than argumentative discussion, for which I have neither the necessary talent, nor any very particular inclination. No answer or reply will be given, to which I am not prepared to append a proof in support of its meaning—this being, in my opinion, the surest mode of conducting a defence.

The circumstances which induced the reviewer to notice my work are stated by him to be these:

1. "The first part of it (the work) affects scientific arrangement; and the subject of which it treats *was first brought before the British public*, in this Journal."

2. "We wish to point out *an error or two* into which the Doctor has fallen."

* The reader will have seen, from his reply to my letter given above, in what manner Mr. Brande has redeemed his implied pledge.

3. "And

3. "And to advertise him of *two or three samples of bad taste* which have probably escaped his notice."

To which I then reply, *seriatim*.

Reply 1. The purely scientific part of the subject of Hydrocyanic Acid was NOT first brought before the British public in the Quarterly Journal of Science; neither was its application to the purposes of medicine first adverted to in that Journal.

PROOFS. A m[illegible]ly account of the discoveries and investigations o[illegible] Lussac on that subject was given in the Annals of [illegible]ophy for December 1815, at which epoch the Quarterly Journal was NOT in existence! And a paper *specifically written for the British public*, respecting the use of Prussic Acid as a medicine, was inserted in the Medical Repository, two years and a half before the subject was noticed in the Quarterly Journal.

COROLLARY. O's first asseveration, therefore, is "incorrect."

Reply 2. Of the one or two errors into which I am said to have fallen by the reviewer, *one only* is mentioned, after all, by him in the course of his critique at page 402, respecting the specific gravity of prussic acid; and that is NOT an error of the author, but a typographical fault.

PROOF. See the *errata corrige* prefixed to my work, in which that fault is actually rectified.

COROLLARY. O's first accusation against me, therefore, is "unjust."

Reply 3. Of the two or three samples of bad taste of which O was anxious to "advertise me," one only is brought forward by him in his review—and that one sample of bad taste is only made to appear as such by artfully coupling together two short garbled quotations from my book.

PROOF. The anecdote related in my book, from which the quotations of the reviewer are taken, relates to a matter of fact, which he has not dared to repeat; or what he has called "bad taste," would have appeared to be "plain truth."

COROLLARY. O's conduct, therefore, is "wanting in candour."

After sketching a short and rapid account of the history of prussic acid, taken in some instances, *verbatim*, though without acknowledging it, from my work, the reviewer proceeds to assert, page 401, that,

1. I have adverted to the history of that substance *superficially*.

2. That I have given the different processes for preparing the acid *without sufficient remarks upon their principles*.

3. That I have passed judgement upon the merits of those processes, *not always tempered with mercy*.

To

To which assertions I beg to apply the following answers:

Answer 1. The chemical history of prussic acid is NOT adverted to superficially in my treatise. It is more fully given, than in many recent works on chemistry. It is a hundred times more extended than that substituted by the reviewer for the edification of his readers.

PROOFS. My chemical history of Prussic Acid begins from the discovery of Prussian blue, and terminates with the latest researches respecting the component parts and real nature of the acid itself, by Gay-Lussac, whose notions and atomic theories are fully given; while the intermediate epochs of this interesting history are duly noticed, the labours of several eminent chemists, particularly those of Morveau, Scheele, Berthollet, &c. as well as those of M. Porrett, in this country, are mentioned; and continued reference made throughout the section, as well as at the conclusion of it, to various works in which the subject has been treated in the most satisfactory manner. The historical account above alluded to occupies *twenty-four* printed pages of my work; while not one-third of that space, in the most approved modern works on chemistry, has been dedicated to that subject, excepting in the laborious and classical system of chemistry by Dr. Thomson. The chemical history of the same substance, substituted by the reviewer himself, occupies just *twenty-nine lines*.

COROLLARY. O's assertion, therefore, respecting my "superficiality," is incorrect.

Answer 2. It is NOT true, that I have given the different processes for preparing the acid "without any sufficient remarks upon their principles."

PROOFS. My description of Scheele's process, written with what perspicuity I could master, is followed up by a complete *rationale* of the various steps of that process, which had been ambiguously interpreted by others! Of Vauquelin's process I observed that its simplicity would render any remark of mine upon it an act of supererogation —and on the third, or Majendie's process, consisting in the mere dilution of the concentrated acid with water, I made sufficient remarks to attract the notice of the reviewer, who has grounded upon them a whole and long paragraph concerning their pretended incorrectness!

COROLLARY. O's assertion, therefore, is again, in this instance, "unjust."

Answer 3. The judgement I passed on the different processes, is so far from being "not tempered with mercy," that in

two

two cases it is given in a strain of eulogium—and in the third no judgement is given at all!

PROOFS. Speaking of that of Scheele, I state, "By this method the acid is obtained at an uniform degree of concentration;" and again, "this acid is perfectly good for the purpose of practice; while, on the subject of that of Vauquelin, I affirm, "that the acid prepared according to his method is of a proper strength for medicinal purposes."

COROLLARY. The assertion, therefore, in this case, is again "wanting of candour."

And here I cannot help observing to you, my dear sir, that this advocate for mercy so far forgets his own precepts, that throughout the review of my work sentiments are uttered and expressions used that are very distantly allied, indeed, to that heaven-born virtue. As a proof of this assertion, I need only mention that the very processes of two such eminent chemists as Scheele and Vauquelin, which he affects to defend from my unmerciful judgement, are, by him, dismissed in the most peremptory language of condemnation—the one as furnishing an acid of "variable composition," the other for being "extremely objectionable."

Speaking of the very curious but difficult branch of chemical inquiry, respecting the formation of prussic acid by the combination of animal matter, contained in the third section of my book, the reviewer says, that "I should either not have meddled with it, or given a clear epitome of what is known upon the subject." In answer to which I have to observe,

1st. That I have NOT meddled with the subject, which is still involved in absolute obscurity, any further than by repeating an uncontroverted fact, which, instead of being quoted in a garbled manner, should have been fairly transcribed by the reviewer: and 2dly, That the whole of what is known on the subject of the formation of Prussic Acid by the combination of animal matter, *is* detailed in the said section, although that *all* be but little, and betray "a poverty in the land," as the reviewer poetically expresses it. And I challenge him to point out any book on chemistry in which more, or even as much, is to be found on that subject, that is not conjectural.

PROOFS. The very paragraph with which the section in question of my work begins, and which the reviewer imperfectly quotes, as unintelligible, is given in the identical language of Berthollet, from whose *Essai de Statique Chimique* it was collected—neither Thomson, nor Murray, nor Henry, nor Klaproth, nor Orfila, nor Lagrangé, no— nor even yourself, in your manual, have alluded to the in-

quiry in question in the slightest degree: so that the "epitome of what is known," may be well comprehended in the two pages that I have dedicated to that subject.

COROLLARY. The reviewer, therefore, is on the present occasion, as on all the preceding ones, wanting in "justice" as well as "candour."

But I have done more:—I have given an extract from my notes, taken while attending a course of lectures on animal chemistry, delivered by Vauquelin, in which an endeavour is made to place the above subject in a somewhat clear light. Yet what is the conduct of O on this occasion? With the same *nonchalance* with which he had, just before, talked of the *unintelligibility* of two paragraphs borrowed from Berthollet and Thenard, he assures his readers, that the extract from Vauquelin's lectures is not a tittle more intelligible; and dismisses it without any quotation in support of his affirmation. Now, as I mean, throughout this letter, to substantiate by PROOFS what I advance on the score of O's candour and justice; and as I hesitate not to assert, that the charge of unintelligibility against Vauquelin is as gross a defection from truth, as that which characterizes the charge brought against his process for preparing the hydrocyanic acid, I shall beg leave to quote, once more, the passage in question, in order that the reader may judge, whether or not it be *unintelligible*. "When animal substances are exposed to heat with a mixture of alcalics—hydrogene, carburetted, and carbonic gas are obtained, besides a *residuum*, which, if washed in water, will be found to contain prussic acid. The alcali, therefore, seems necessary to form the prussic acid, by attracting together the principles of which it is constituted," &c.* If this be unintelligible, then plain language is not capable of expressing common ideas; but if, on the contrary, the passage be found perfectly comprehensible, and such, indeed, as will be met in substance, in the works of most men of eminence who have written on the same subject, then the conclusion regarding the reviewer's candour is unavoidable.

The reviewer's charge, at page 406, of my having unnecessarily separated the account of the physical properties of the prussic acid,

* It is not a little curious, that one of the most important works on Chemical Science, written in the English language, should contain a passage nearly similar in import to the above, on the subject of Prussic Acid. Having described the process of heating blood and alcalies, to procure the acid —the author proceeds to give the following explanation of that process: "This process consists essentially of two operations, one the impregnation of the alcali with that peculiar principle contained in the blood, which gives the power of striking a blue colour with iron, and is called the prussid acid," &c. Vide Aikin's Dictionary of Chemistry, Art. Prussian Blue.

acid, from its chemical history and preparation, is of too trivial a nature, and absurd, to need refutation. Every book on chemistry is full of examples of such practice; and Gay Lussac himself has followed no other method in his admirable essay on hydrocyanic acid.

In the succeeding paragraph of his critique, relating to the physiological experiments made with the *pure hydrocyanic acid* by several authors and myself, the reviewer remarks, that "as Mr. Brodie's investigations *upon this subject*, are the most satisfactory that have hitherto been made; and, as they are not even alluded to by me, I shall decline troubling his readers with those I have detailed." To which, this is my reply:

1. Mr. Brodie NEVER made any experiment with the *pure hydrocyanic acid.*
2. Previous and subsequently to Mr. Brodie's investigation respecting the action of various poisons on animals, Coullon, Emmert, Magendie, and others, had and have instituted experiments with the pure hydrocyanic acid, or with substances containing it, not only upon animals, but upon the human system, which, in a work of practical utility, and not simply of philosophical speculation, could not but be preferred to every other experiment.

Either, therefore, O knew all this; and in such a case, where is candour and truth in concealing it?—or he knew it not; and in that case, it was his duty, ere he undertook to criticise the book, to have made himself master of its subject.

The eighth section of the work relates to the means of detecting prussic acid, and preventing its poisonous effects: "in neither of which," says the candid and just reviewer, "do we remark any thing either very new or very important;" but respecting which I must beg leave to ask him two questions.

1. Is it not *very important* to determine the symptoms of poisoning by this acid, and to ascertain the best means for counteracting its deleterious effects? These objects have been accomplished, as far as they could be, in the said eighth section.

2. Is it not *very important* to be acquainted with the means of detecting the presence of prussic acid, particularly in cases of death from that substance? And have these means, or the mode of conducting the investigation, been pointed out to the public before the appearance of my work, by any chemist, English or foreign? Is not that new, which is not to be found elsewhere? Is any thing of the kind contained in the works of Fourcroy, Chaptal, Thenard, Thomson, Murray, Orfila, Henry, Children, or even in your own manual of chemistry? No.

Then what becomes of the justice, correctness, candour, and I may now add, the knowledge, in these matters, of the reviewer?

 Perhaps

Perhaps some evidence of all these qualities is to be found in the remaining part of the Review, relating to the most important as well as to the largest portion of my work, which is dismissed in ten lines and a half!

I am now arrived at that part of my reply, upon which I enter with feelings of great reluctance; because it alike involves charges of a heavy nature against the reviewer; and obliges me, from a sense of what is due to truth, publicly to deny the correctness of opinions said to be your own.

My charges against the reviewer are, 1. Misrepresentation, or concealment of facts. 2. Ignorance of the subject on which he has undertaken to pronounce. 3. Unworthy insinuations against the author, whose book he reviews. Each and all of which charges, in pursuance of the plan I have followed throughout this reply, I shall proceed to substantiate by positive proofs: leaving, however, to the public, the task of drawing, in this instance, the corollaries that must necessarily follow.

The first charge, or that of misrepresentation or concealment of facts, is supported by the following evidence:—1. The reviewer tells his readers that the formula of Dr. Magendie for diluting Gay Lussac's acid, is not given in my book (p. 402); whereas the *fact* is, that at page 20 of my Treatise *I have inserted that formula* thus: "Dr. Magendie dilutes the concentrated acid of Gay Lussac with six times its volume, or eight times and a half its weight of distilled water."—2. The reviewer, in the same paragraph, informs his readers that the number 9.20583 is quoted by me as the "medium density" of Magendie's diluted acid; whereas the truth is, that I distinctly used the word *weight*, meaning the absolute weight, and not the "*medium density*" of a mixture of 8.5 of water and 0.70583 of concentrated acid.—3. The reviewer says, I have "fallen into some sad errors respecting the specific gravity of the pure acid," and merrily and triumphantly quotes a typographical fault by which I am made absurdly to state the specific gravity of the acid to be 70.583; whereas in the *errata*, which every candid reviewer would have turned to on seeing such an absurd mistake, the printer's error is actually rectified, and the specific gravity correctly given thus, .705 3.—4. The reviewer states, "that the doctor insinuates, though he must know better, that the acid sold at Apothecaries' Hall is *always* turbid, yellowish and *impure*." This is NOT true. The *Doctor* never expressed such an insinuation; nor did he use the two words "always" and "impure;" which the reviewer, with utter disregard to propriety, has attributed to him, and has even marked in italics. The following is the only passage in which I passed any degree of condemnation on the acid prepared at Apothecaries' Hall: "I know, besides, that the acid thus prepared

pared is of a turbid yellowish colour, instead of being colourless and transparent, and that it deposits a considerable sediment; both which circumstances *seem* greatly to militate against its *purity*."

The second charge against the reviewer, or that of ignorance of the subject on which he has undertaken to pronounce, is thus substantiated. 1. The reviewer states at page 400, that he is "not quite clear" whether Gay Lussac gave the name of *cyanogene* to the base of hydrocyanic acid "because it burns with a bluish purple flame, or because it is essential to the production of Prussian blue;" whereas, Gay Lussac is positive as to the latter reason; and every chemist acquainted with the subject knows it to be so.—2. The reviewer asserts, that the acid obtained by Scheele's method, "is of variable composition, because Prussian blue is not always of equable purity:" but this is not true in practice. Mr. Garden has prepared prussic acid according to this method for the last three years and a half, with an invariable precision of result and equality of strength, which, to judge of its constant specific gravity, is stronger than that prepared at Apothecaries' Hall.—3. The reviewer has acknowledged his inability of obtaining *pure* prussic acid by Vauquelin's method, which, he asserts to have tried *frequently*. But the want of success in his case must be ascribed to his ignorance in chemical manipulations, *as you well know*; for you *have seen* pure prussic acid prepared according to that method.—4. The reviewer says he cannot understand the following passage of my work, containing the well known hydrodynamic axiom, that "the weight of fluids is equal to their volumes multiplied by their densities." I can only observe, in reply, that if he is a stranger to mathematical language, he has only to refer to Dr. Young's lectures on Natural Philosophy, or Biot's, or Brisson's, or any other author's work on the same subject, in all of which that very axiom is mentioned and explained. In the instance above alluded to, the passage was translated *verbatim* from a note in Dr. Magendie's pamphlet on the subject of which I was commenting.—5. The reviewer, making himself quite merry, and rioting in the pleasures of the discovery of the "Doctor's sad errors," asserts that the specific gravity of a mixture of six volumes of water at 1, and one volume of acid at 0.70583, such as Magendie employs, *must* be 0.9900: whereas, any person acquainted with the common rules of alligation, would know that $\frac{6\times1+0.70583}{7} = 0.9579$. To which I add that such is nearly the actual specific gravity of the mixture in question, ascertained by repeated experiments—the "increase of density resulting from the mixture of the pure acid

acid with water" not being so "great" as the reviewer boldly asserts.

The third charge against O, namely, that of throwing out unworthy and unwarrantable insinuations against me, will be best substantiated by a quotation of his own expressions. "We should have conceived it more decorous on the part of Dr. Granville, finding the above preparation objectionable, as he has asserted it to be, to have stated the objections to the Apothecaries' Company, *instead of publishing their process with a view to depreciate it, and to employ it as a vehicle of a puff-oblique in favour of the Doctor's chemist, Mr. Garden.*" I despise too much the individual, who, without the slightest degree of evidence in support of them, can assume and publish two such inferences, and direct them against the moral rectitude of an author—to be disposed to take any other notice of the above disgraceful insinuations against my character, than to express my utter astonishment, that you should have suffered it to appear in your Journal! Did you know of any thing in my conduct during the six or eight years of our acquaintance; or any passage in my work which could have led you to admit, for one instant, the propriety of the aspersions of my reviewer? The only passage that relates to Mr. Garden the chemist, and which is contained in the same paragraph of my work, which seems to have given such mortal offence to O, is this: "I have not had an opportunity of trying this acid (the Apothecaries') as I am satisfied with that which Mr. Garden prepares for my patients." Is *truth*, then, synonymous with *puff-oblique* in the moral lexicon of my reviewer?—As to its being or not "*more decorous*" for me to have stated the objections I entertained against their acid, to the Apothecaries' Company themselves, instead of publishing those objections; I have yet to learn that that worshipful body have any claim to the services of any physician. Let them look to their own business; and see that their own officers do their duty. O gives a sort of manifesto in his review, from the Apothecaries' Company, in which they declare, that they "*have no secrets;*" if so, why should he feel sore at my having published their process of preparing the hydrocyanic acid? and how, I would furthermore ask him, can the publication of such a process "*depreciate it,*" as he rather awkwardly states, if the process be inherently perfect?

A few more words on the subject of certain opinions ascribed by the reviewer to yourself; and on the defence set up by that gentleman in favour of the Apothecaries' acid of 1819 and 1820, and I conclude.

You are said in the Review, to have reported to the Laboratory Committee of the Apothecaries' Society that, "having tried the

the methods of Scheele and Vauquelin, you found them uncertain in their products, and more especially in the latter case, the specific gravity of Vauquelin's acid *always* exceeding that of distilled water." In opposition to which opinions, I assert, 1. That Scheele's method, with common precaution, yields, by far, the most equable and the purest acid for medicinal purposes, of a specific gravity inferior to that quoted by you as the density of the acid prepared according to your own formula, and which is stated to be 0.995, although I have found it to be as high as 0.998 in two specimens procured at the Hall a few weeks back—whereas, 0.993 is the invariable density of Scheele's acid at an uniform temperature. 2. That by Vauquelin's method, prussic acid, possessing all the requisite physical and medicinal properties, colourless, transparent, and of the specific gravity of 0.998, and, consequently, *under the standard density* of distilled water, can be procured—is annually procured to a large amount by the French chemists, who sell no other—and has been procured by me at two different times, since your assertion to the contrary has been published by the reviewer. You have not, I suppose, forgot, that, on the evening of the 18th ultimo, I showed you, at the table of the Royal Society, a specimen of the acid so prepared by me, as well as another, prepared, according to Scheele's process, by Mr. Garden, which you admitted to be "as good specimens as could be desired, and the purity of which, to judge of their specific gravity marked on the phials containing them, seemed to be quite unobjectionable." It was on that same occasion, that I remarked to you, that the accuracy of Vauquelin was too well established, to allow us for a moment to suppose, that he would have recommended a process which, according to your expressed opinion, "is uncertain in its products," and the specific gravity of which "is *always* greater than that of distilled water." To the justice of this remark you then assented. The specimens alluded to were shown that same evening, and in the same place, to Dr. Holland and Messrs. Philips and Faraday, to whom, as well as to you, I exhibited two other specimens of the acid procured at Apothecaries' Hall in 1819 and 1820. One of these presented a fluid of a dark-brown colour—turbid, when shaken—but transparent when suffered to rest so as to give time for a copious blackish sediment to subside: the other offered a fluid of a muddy colour and appearance, though by no means so striking as in the former case. From these two specimens, obtained at two different periods from the Hall, I then declared to you, and to the other gentlemen above mentioned, what I again repeat on the present occasion, that the description of the Apothecaries' acid, contained in the second edition of my work, was taken; and I may, I think, call upon you to say, whether under such

such circumstances, I should not have been justified in passing a *severer* judgement upon the merits of that acid, than that which my reviewer has qualified by the expression, "not tempered with mercy;" and to which I omitted to advert in another part of this reply, because I considered the present as a fitter opportunity for so doing.

The fact is, that both you and the reviewer—the one verbally, in conversation with me—the other in writing, at page 404 of his Review, have admitted the justice of my remark as to the appearances of the acid prepared at Apothecaries' Hall; and it remains only for me to say, in conclusion, that if my reviewer can assert, as he has done at page 404, that "the occasional yellowness and turbid appearance of that acid is *rather an indication of its purity than otherwise,*" his logic will certainly fail to convince his readers, as it failed to convince myself. If, however, by the admission that the acid is occasionally yellow and turbid, and presents, moreover, a sediment, its decomposition is also admitted (which the reviewer has virtually done); then a preparation liable to such fatal objections, ought not to have been defended as a "very uniform and very pure product;" nor sold as *pure prussic acid* by the Apothecaries' Company, to supply "the occasional demand" for that article, as they have unquestionably done in the case of the two specimens in my possession, which were nearly as objectionable in their appearance when first procured, as they are now, when "age" and "purity," says the reviewer, are to be considered as the two causes of those objectionable appearances. For the information and caution of every medical practitioner, I publicly exhibited, last night, at a meeting of the Medico-Chirurgical Society, the two latter specimens, as well as those alluded to in the course of this letter; which have been prepared agreeably to Scheele's and Vauquelin's methods—and will show them to any other member of the profession, who may be desirous of inspecting them*.

As to the tone and style, generally, in which O's review is written, I can only assent to the judgement passed upon them, in a scientific circle, by one of the first philosophers in the coun-

* The relative value of the acid prepared by Mr. Garden, and at the Hall according to Mr. Brande's formula, will be further illustrated by the following facts. A pupil of St. Bartholomew's assured me the other day, that the Apothecaries' acid had been administered to patients in that hospital, in doses of *twenty-four* drops at a time, without the slightest obvious effect. Mr. Travers mentioned to the Medico-Chirurgical Society, a few nights since, that one of the servants of St. Thomas's Hospital, having inadvertently swallowed a mixture containing about eighteen drops of prussic acid, prepared by Mr. Garden, which he mistook for an aperient draught, fell down on the floor as if shot by a cannon-ball! and continued ill for some

try *; to whose opinion both you and I must readily bow—which judgement, given in the presence of two or three other gentlemen, but addressed to me in particular, went to condemn both as equally derogatory from the dignity of a reviewer, and injurious to the journal in which such a review has been admitted.

I have the honour to be your humble servant,

Saville-row, Feb. 7, 1821. A. B. GRANVILLE.

* This last paragraph has undergone a trifling verbal alteration, since the manuscript was submitted to Mr. Brande, and rejected—in consequence of that gentleman having erroneously interpreted its original meaning, in his note to me, and also with a view to avoid any such misinterpretation.

XVIII. *Geocentric Places of Vesta and Ceres; and the apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for March and April* 1821.

To Mr. Tilloch.

Cirencester, Feb. 12, 1821.

SIR, — IN compliance with your wish that (on account of your more distant readers) all notices of astronomical phænomena might be published *two* instead of one month in advance, I send you the geocentric places of Vesta and Ceres for March and April. I have also added the apparent right ascension of Dr. Maskelyne's 36 stars for the noon or beginning of every day of the same months. The mean places were deduced from Mr. Pond's table annexed to the Nautical Almanac for 1823, and the corrections carefully calculated from Dr. Maskelyne's own tables. I remain, sir,

Your obedient servant,

JAMES GROOBY.

Geocentric Places of Vesta and Ceres.

1821.	VESTA		CERES.	
	Rt.Asc.	Declin.	Rt.Asc.	Declin.
March	H. M.	° ′	H. M.	° ′
1	7 9	26 14 N.	16 19	14 5 S.
7	7 10	26 19	16 22	14 16
13	7 11	26 22	16 26	14 24
19	7 13	26 21	16 30	14 30
25	7 18	26 19	16 32	14 34
April 1	7 22	26 13 N.	16 33	14 37 S.
7	7 28	26 3	16 33	14 40
13	7 34	25 51	16 32	14 43
19	7 41	25 37	16 29	14 45
25	7 48	25 22	16 27	14 46

1821	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37
Mar.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	1·43	6·22	56·30	40·55	30·29	57·44	0·41	30·44	16·88
2	43	21	29	53	27	42	40	43	86
3	43	20	28	52	25	40	38	42	85
4	43	19	27	50	23	39	37	40	83
5	44	18	26	49	21	37	35	39	82
6	44	17	25	47	19	35	33	37	80
7	45	16	24	46	17	33	31	35	79
8	45	15	23	44	15	32	30	34	77
9	45	14	22	42	12	31	29	32	75
10	45	13	21	41	10	29	27	31	74
11	45	12	20	39	08	27	25	29	72
12	45	11	19	38	06	25	23	27	71
13	46	11	18	37	03	24	21	26	69
14	46	10	17	35	01	22	20	24	68
15	47	09	16	34	29·98	21	18	23	66
16	47	09	15	32	96	20	17	22	64
17	48	08	14	30	94	18	15	20	62
18	49	08	13	29	92	16	13	18	60
19	49	08	12	28	90	15	12	16	59
20	50	08	11	27	88	13	10	15	58
21	50	07	10	25	86	12	08	13	56
22	51	07	10	24	84	10	07	12	54
23	52	07	09	22	82	09	05	11	52
24	53	07	08	21	80	07	04	09	50
25	54	06	07	19	78	05	02	08	48
26	55	06	07	18	76	04	00	06	46
27	56	06	06	17	74	02	14·59·98	04	44
28	57	05	05	15	72	00	96	03	42
29	57	05	04	14	70	56·98	95	01	40
30	58	04	03	13	68	97	93	00	38
31	59	04	02	12	67	95	92	29·99	36

1821.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Mar.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	12·13	57·57	23·29	49·50	52·22	57·79	24·56	48·37	31·94
2	12	56	28	50	22	80	57	37	96
3	11	55	27	50	22	81	59	39	99
4	10	54	26	50	23	82	60	40	32·01
5	08	53	25	49	23	83	61	43	04
6	07	53	24	49	23	84	62	45	06
7	05	51	23	48	23	85	63	47	09
8	04	50	22	48	23	86	64	49	11
9	03	48	21	47	23	87	65	51	13
10	01	47	19	47	23	88	66	53	16
11	00	46	18	46	23	89	67	55	18
12	11·98	45	17	46	23	90	68	57	20
13	97	43	16	45	23	90	69	59	22
14	95	42	14	45	23	91	70	60	24
15	94	41	13	44	22	91	70	62	26
16	93	40	12	43	22	92	71	63	28
17	91	39	10	43	22	93	71	65	30
18	90	38	09	42	21	93	72	66	32
19	88	37	07	42	21	94	72	68	34
20	87	36	06	41	21	94	73	69	36
21	85	35	04	40	20	95	73	71	38
22	83	33	03	39	19	95	74	72	40
23	81	32	02	38	19	95	74	74	42
24	80	30	00	37	18	96	74	75	44
25	78	28	22·98	36	17	96	75	76	46
26	77	27	97	35	17	96	75	77	47
27	75	25	95	33	16	97	75	79	49
28	73	23	93	32	15	97	75	80	50
29	72	21	91	31	15	97	76	81	52
30	70	20	90	30	14	97	76	82	53
31	69	18	89	29	14	97	76	83	55

1821.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares	α Herculis	α Ophiuchi	α Lyræ	γ Aquilæ
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37
Mar.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	49·75	1·19	8·21	28·81	26·90	30·20	38·47	52·67	45·01
2	78	22	24	84	93	23	50	70	03
3	80	24	27	87	97	27	53	73	06
4	83	27	30	90	27·00	30	56	77	08
5	85	29	33	93	04	33	59	80	11
6	88	32	36	96	07	36	62	84	14
7	91	35	39	99	11	39	65	87	16
8	93	37	42	29·02	14	42	68	90	19
9	96	40	45	05	17	45	71	94	21
10	98	42	48	07	21	48	74	97	24
11	50·01	45	51	10	24	51	77	53·00	26
12	03	47	53	13	28	54	80	03	29
13	06	50	56	15	31	57	83	07	31
14	08	52	58	18	34	59	86	10	34
15	11	55	61	20	37	62	89	13	36
16	13	57	63	23	40	65	92	16	39
17	16	60	66	26	44	68	95	20	42
18	18	62	68	28	47	71	98	23	44
19	21	65	71	31	50	73	39·01	27	47
20	23	67	73	34	54	76	04	30	49
21	26	70	76	37	57	79	07	34	52
22	28	72	78	39	60	82	10	37	55
23	30	74	81	41	63	85	13	41	58
24	32	76	83	44	66	88	16	44	61
25	34	78	86	46	69	91	19	47	64
26	36	80	88	48	72	94	22	50	67
27	38	82	91	50	75	97	25	54	70
28	40	84	93	52	78	31·00	28	57	73
29	42	86	95	55	81	03	30	60	75
30	44	88	98	57	85	06	33	64	78
31	46	90	9·00	59	88	[illegible]	36	67	81

1821.	α Aquilæ	β Aquilæ	1α Capri.	2α Capri	α Cygni	α Aqua.	Fomalhau.	α Pegasi.	α Andromedæ
	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Mar.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	2·97	31·26	43·24	6·99	18·79	35·02	44·14	50·63	8·67
2	99	28	26	7·01	81	04	15	64	67
3	3·02	31	29	04	84	06	16	65	67
4	04	34	32	07	86	07	17	66	67
5	07	37	35	10	89	08	18	67	67
6	09	39	37	12	91	09	19	68	67
7	11	41	40	15	94	11	20	69	67
8	14	44	42	17	97	12	21	70	67
9	16	46	44	19	99	13	22	71	67
10	19	49	47	22	19·02	14	23	72	67
11	21	51	49	24	04	16	24	73	67
12	24	53	52	27	07	18	25	74	67
13	26	56	55	30	09	19	27	75	68
14	29	58	57	32	12	21	28	77	68
15	31	61	60	35	15	23	30	78	69
16	34	63	63	38	18	24	31	80	69
17	37	66	66	41	21	26	32	81	70
18	39	68	69	44	24	28	34	82	70
19	42	71	72	47	27	30	35	84	71
20	45	73	74	49	30	32	37	85	72
21	48	76	76	51	33	34	39	86	73
22	51	79	79	54	36	36	41	88	74
23	53	81	82	57	39	38	42	89	75
24	56	84	84	59	42	40	46	91	76
25	59	87	87	62	45	42	46	92	77
26	62	90	90	65	48	44	48	93	78
27	65	93	93	68	52	46	50	95	79
28	68	96	96	71	55	48	52	96	80
29	71	99	99	74	59	50	54	98	81
30	73	32·01	44·02	77	62	52	56	51·00	82
31	76	04	05	80	65	54	58	51·01	83

1821.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel	β Tauri	α Orionis.	Sirius.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 14	H. M. 5 45	H. M. 6 37
April	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	1·60	6·04	56·02	40·11	29·65	56·94	59·91	29·98	16·35
2	62	04	01	10	63	92	89	96	33
3	63	04	01	09	61	91	88	95	32
4	64	04	01	08	59	90	87	93	30
5	66	05	01	07	58	88	86	92	28
6	68	05	01	06	56	87	84	90	26
7	69	05	01	05	54	86	83	89	24
8	70	06	01	04	52	84	81	88	23
9	72	06	00	03	51	83	80	87	21
10	73	07	00	02	49	81	79	85	18
11	74	07	00	01	48	80	78	84	16
12	76	08	00	00	46	79	77	82	15
13	78	08	00	39·99	45	77	75	81	13
14	80	09	00	98	43	76	74	80	12
15	81	09	00	98	42	75	73	79	11
16	83	10	55·99	97	40	74	72	77	09
17	84	11	99	96	39	73	71	76	07
18	86	12	99	96	37	71	70	75	05
19	87	13	99	95	36	70	69	74	04
20	89	14	56·00	94	35	70	68	73	02
21	91	15	00	94	34	69	67	72	01
22	93	16	01	93	33	68	66	71	15·99
23	95	17	01	93	32	68	65	71	98
24	97	18	01	92	31	67	64	70	96
25	2·00	19	01	92	30	66	64	69	95
26	02	21	02	92	29	65	63	68	94
27	04	22	02	92	28	64	62	67	93
28	07	23	02	91	27	64	62	67	92
29	09	24	03	91	27	63	61	66	90
30	11	25	03	91	26	63	60	65	89

1821.	Castor.	Procyon.	Pollux.	α Hydrae.	Regulus.	β Leonis.	β Virginis.	Spica Virgi.	Arcturus.
	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
April	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	11·67	57·17	22·87	49·28	52·13	57·97	24·77	48·84	32·56
2	65	16	85	27	13	97	77	85	57
3	64	14	84	26	12	97	78	86	59
4	62	13	82	25	11	97	78	87	60
5	60	11	80	24	10	97	79	88	61
6	58	09	78	22	09	97	80	89	63
7	56	08	76	21	08	97	80	90	64
8	55	07	75	20	07	97	81	91	65
9	53	06	74	19	06	96	82	92	67
10	52	04	72	18	05	96	82	93	68
11	50	02	70	17	04	96	82	94	69
12	49	01	69	16	03	96	81	95	71
13	47	56·99	67	14	02	96	81	95	72
14	45	98	66	13	01	95	80	96	73
15	44	96	64	12	00	95	79	96	73
16	42	94	62	10	51·98	95	78	97	74
17	40	93	61	09	97	95	78	97	75
18	39	91	59	08	96	94	77	98	76
19	37	90	58	07	95	94	77	98	77
20	36	88	56	06	94	94	76	99	78
21	34	87	55	05	93	93	76	99	79
22	33	85	54	03	92	93	75	49·00	80
23	31	84	52	02	90	92	75	00	81
24	30	82	51	00	89	92	74	00	81
25	28	81	49	48·99	88	91	74	01	82
26	27	80	48	98	87	90	73	01	83
27	26	79	47	97	86	90	73	01	83
28	24	78	45	96	85	89	72	02	84
29	23	76	44	94	84	89	72	02	84
30	21	75	42	93	83	88	71	02	85

1821.	1α Libræ	2α Libræ	α Cor Bor.	α Serpentis	Antares.	α Herculis.	α Ophi.	α Lyræ.	γ Aqui.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37
April	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50·48	1·92	9·02	29·62	27·92	31·12	39·39	53·70	45·84
2	50	94	04	64	95	14	41	74	87
3	52	96	07	65	99	17	44	77	90
4	54	98	09	68	28·03	20	47	81	93
5	56	2·00	11	70	07	23	50	84	96
6	58	02	14	73	11	26	53	88	99
7	60	04	16	75	15	28	56	91	46·02
8	62	06	18	77	19	31	59	95	05
9	64	08	20	79	22	33	61	98	08
10	66	10	22	81	26	36	64	54·02	11
11	67	11	24	83	28	38	66	05	14
12	69	13	26	85	31	41	69	08	17
13	70	14	28	87	34	43	71	12	19
14	72	16	30	89	37	46	74	15	22
15	73	17	31	90	39	48	76	18	25
16	74	18	33	92	42	51	79	21	28
17	76	20	35	94	44	53	81	24	31
18	77	21	37	96	47	56	84	28	34
19	79	23	38	98	49	59	86	31	37
20	80	24	40	30·00	52	62	89	34	40
21	81	25	41	02	54	64	91	37	43
22	83	27	43	03	56	66	93	40	46
23	84	28	44	05	59	69	96	42	49
24	85	29	46	07	61	71	98	47	52
25	86	30	47	09	63	73	40·00	50	55
26	88	32	48	10	66	75	03	53	58
27	89	33	50	12	68	78	05	56	61
28	90	34	51	14	71	81	07	59	64
29	91	35	53	15	73	83	10	62	67
30	92	36	54	17	76	85	12	65	70

1821.	α Aqui.	β Aqui.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhau.	α Pegasi.	α Androm.
	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
April	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	3·79	32·07	44·08	7·83	19·68	35·56	44·60	51·02	8·84
2	82	10	11	86	72	59	62	04	86
3	84	13	14	89	75	61	65	05	87
4	87	16	17	92	79	64	67	07	89
5	90	19	20	95	82	66	69	09	90
6	93	22	23	98	85	69	71	11	92
7	96	25	26	8·01	89	71	73	13	93
8	98	28	29	04	93	73	76	15	95
9	4·01	31	32	07	96	76	78	17	97
10	04	34	35	10	20·00	78	81	19	98
11	07	37	38	13	03	81	83	21	9·00
12	10	40	41	16	07	84	86	23	01
13	13	43	44	19	10	86	88	25	03
14	16	46	47	22	14	89	91	27	05
15	19	49	50	25	17	91	93	29	06
16	22	52	54	29	21	94	95	32	08
17	25	55	57	32	24	96	98	34	10
18	28	58	60	35	28	99	45·00	36	12
19	31	61	63	38	31	36·02	03	38	14
20	34	64	67	42	35	05	06	40	16
21	37	67	70	45	39	08	09	42	18
22	40	70	73	48	42	10	12	45	20
23	43	73	76	51	46	13	15	47	22
24	46	76	79	54	50	16	18	50	24
25	49	79	82	57	53	19	21	53	26
26	52	82	85	60	57	22	24	56	29
27	55	85	89	64	61	25	27	58	31
28	58	88	92	67	65	28	30	61	33
29	61	91	95	70	69	30	33	63	36
30	64	94	98	73	73	33	36	66	38

XIX. *On the Visibility of the Planet Venus during the Day, and the Use that may be made of this Fact in determining Longitude.*

To Mr. Tilloch.

January 18, 1821.

Sir,—It is very well known that some of the planets, and the fixed stars to the second or third magnitude, may be seen in full daylight with *but small telescopes*; but I am not certain whether it is generally known, that Venus can be seen during a great portion of her revolution around the sun, in full sunshine with *the naked eye*. As the Moon's distance from the planets may at some future period be employed in deducing the longitude at sea, and Venus is easily found in the daytime, she may be of more service in that operation than those planets which are slower and less bright. If you think the following extract from my diary worth insertion in the Philosophical Magazine, as bearing upon this point, I shall be much obliged by your making use of it. It appears to me, that if ever Venus is employed in deducing the longitude, she affords a ready means of extending the advantage of taking the moon's distance from a planet, through a great portion of the day, when it might not have been expected.

I am, Sir,

Your very obedient humble Servant,

β.

1820.		h.	m.	
May	16	1	0 P.M.	Venus near the ☽. ☉ shining very bright; seen with *the naked eye;* with power 40 was distinctly seen through flying clouds which *obscured the* ☽; also distinct with 170.
June	24	12	0 Noon.	Again in bright sunshine, *with naked eye.*
July	10	9	30 A.M.	Ditto ditto ditto
	15	2	0 P.M.	Ditto ditto (about 23° dist. from ☉)
	19	10	45 A.M.	Ditto, with power 40 distinct, but *not* with naked eye.
	22	11	0 A.M.	Ditto ditto through flying clouds, but *not* with naked eye.
	25	9	0 A.M.	Ditto very beautiful with 40 and 90, ditto.
	26	1	0 P.M.	Ditto ditto 70, ditto.
	31	12	0 Noon.	Ditto very bright, with 40 and upwards, ditto was in ☌ yesterday.
August	5	9	30 A.M.	Ditto, very distinct with 40 and upwards, *not* with naked eye.
	7	9	45 A.M.	Ditto ditto ditto ditto.
	13			Ditto ditto passing merid. ditto.

30

1820.	h. m.	
Aug. 30	1 0 P.M.	Ditto ditto *with naked eye* (about 37° from ⊙).
Sept. 3	10 30 A.M.	Ditto ditto, with 170, and ditto.
4	12 30 P.M.	Ditto ditto ditto.
7	during ⊙ eclipse.	Ditto rather brighter than at other times, *with naked eye*.
10	9 15 A.M.	Ditto very bright, through cirrhi, ditto.
11	12 30 P.M.	Ditto ditto on a clear sky, ditto.
12	12 30 P.M.	Ditto (appearances of inequalities with 170) ditto.
Oct. 1	9 30 A.M.	Ditto exceedingly bright, ditto.
3	10 45 A.M.	Ditto very bright near the ☽, both do.
28	11 0 A.M.	Ditto ditto ditto.
Nov. 3	11 0 A.M.	Ditto ditto ditto.
19	11 30 A.M.	Saw ♀ again very bright in the telescope with 40. *difficult to find with naked eye* (about 43° from ⊙).
Dec. 12	2 0 P.M.	Saw Jupiter near the ☽; also his belts in full sunshine with 40; middle bright belt much brighter than the rest of the planet, which was very pale, but pretty well defined; he was *not so bright as the moon.*
1821.		
Jan. 16	9 30 A.M.	Saw ♀ again in bright sunshine, *invisible to the naked eye*, very white, pale, and indistinct with 170, but with 40 much better; alt. about 15°; she was visible through thin flying clouds.

Thus she was visible to the unassisted sight from a period anterior to 16th May, when I first saw her, until 15th June; from that time, till about the 30th August, she was too near the sun to be seen without better eyes than mine, unless assisted by a telescope, when she again became visible to the naked eye, and continued so until about the 19th November: and, I may add, at various altitudes above 10°, below which, I am not certain I have seen her during sunshine.

XX. *Notices respecting New Books.*

M. Wronski and Dr. Young.

To Mr. Tilloch.

Sir,—The attention which has lately been paid in the Philosophical Magazine to astronomical subjects, entitles its Editor's opinion

opinion of the Nautical Almanac to a certain degree of respect. He is therefore requested to state on what authority he has asserted, in the last Number, that "Dr. Young has *publicly* acknowledged a blunder in his Postscript on Refraction, published in the *Nautical Almanac for* 1822."

London, Feb. 10, 1821.

The above request, though anonymous, is entitled to an answer, because the article alluded to (namely, the Notice respecting M. Wronski's *Address to the Board of Longitude*, which appeared in our last Number) was incorrect. Dr. Young's acknowledgement was not *publicly* made, but was transmitted to M. Wronski in a *private* letter, in his capacity of secretary to a *Public Board*; and was published by the gentleman to whom it was addressed. Our mistake, we beg to say, was not intentional: the acknowledgement was before the public when we wrote the article alluded to; and, having been made by Dr. Young, the fact for which we noticed it is no way affected by our inadvertency. Of this the public will judge from the following extract from M. Wronski's "*Advice*" prefixed to his *Address*:—

"Now, for reasons which will be learned in the present Address, the same [the Theory of Refractions] was returned to us, without even undergoing an examination by the Board; as is proved by Dr. Young's letter, dated the 27th of April, which accompanied the returned Theory. Nevertheless, from the same proposition, some advantage resulted to the Board, who has declared to have perceived by it, that the New *Table of Refractions* which had just been produced in its *Nautical Almanac for the year* 1822, was false; as is proved by the letter of Dr. Young, dated the 18th of April, at the time of the reception of the above theory, where, on acknowledging the same, he confesses immediately this error in the following terms:—

"'I shall give no opinion of my own to the Board, except so far as to acknowledge that you have detected a *blunder* in my hasty Postscript on Refractions.

"'Signed Thomas Young.'"

Our article on M. Wronski's Address was originally of greater length than given in our pages. It concluded with an observation or two on a paragraph in his 96th page, "concerning new mathematical methods promised in the *Nautical Almanac for* 1822, as being to be inserted in the *Philosophical Transactions* for 1819 . . . . . . from Dr. Young, Secretary of the Board of Longitude and of the Royal Society." This paper, which had "for its object astronomical refractions, and according to which has been calculated the Table of Refractions in the Nautical Almanac above cited," did not appear in the *Philosophical Transactions*

actions, but was published separately under the title "*Postscript to Dr. Young's Letter on the Reduction of Experiments ; No.5. Corrections for Refractions.* These are the facts; and in cutting down the article in haste, to bring it within the limits of the page, the mistake occurred to which the foregoing letter of our correspondent alludes.

Lately published.

Observations on the Climate of Penzance and the District of the Land's End in Cornwall; with an Appendix, containing Meteorological Tables, and a Catalogue of the rarer indigenous Plants. By John Forbes, M.D. Secretary to the R.G.S.C. and Physician to the Penzance Dispensary. 8vo. pp. 70.

This work presents much useful matter condensed into a narrow compass. From the facts collected by the author, it appears evident that the temperature of Penzance is remarkable for its small annual, monthly, and daily range, and for its equability from day to day. The mean temperature of the winter there is so high as 42°, while that of summer does not exceed 57° on an average of 14 years; and the mean annual range is only 49°. The lowest degree to which the thermometer has fallen at Penzance during the last 14 years, has been 19°, and this only once.

A Series of Designs for Private Dwellings. By J. Hedgeland. Part I. 4to. 1*l*. 1*s*.

Specimens of Gothic Architecture selected from various Ancient Edifices in England. By A. Pugin, Architect. 4to. 1*l*. 1*s*.

Practical Electricity and Galvanism; containing a series of Experiments, calculated for the use of those who are desirous of becoming acquainted with that branch of science. By John Cuthbertson. 8vo. 12*s*.

General Elements of Pathology. By Whillock Nicholl, M. D. 8vo. 9*s*.

A Synopsis of the various Kinds of difficult Parturition, with Practical Remarks on the Management of Labours. By Samuel Merriman, M.D. 8vo. 12*s*.

Mathematical Essays. By the late W. Spence, Esq. 4to. 1*l*. 16*s*.

Universal Science, or The Cabinet of Nature and Art. 2 Vols. 8vo. By Alexander Jamieson. 16*s*.

Preparing for Publication.

Dr. Granville is preparing Memoirs on the present State of the Sciences in France, containing a description and historical account of the Royal Garden of Plants; the Royal Institute; the Polytechnic School; the Faculty of Sciences; and the Cabinet of Mineralogy; the Public Libraries; the Medical School, and the Hospitals, &c.

Journal of an Horticultural Tour in the Netherlands and North of France, in the Autumn of 1817. By P. Neill, J. Hay, and J. Macdonald, a Deputation of the Caledonian Horticultural Society.

The Substance of the late Professor Dalzell's Lectures on the Ancient Greeks, and on the Revival of Greek Learning, will soon be published by his son, John Dalzell, Esq. Advocate.

Illustrations of British Ornithology. By P. J. Selby, Esq. Elephant folio.

A Dissertation, showing the identity of the Rivers Niger and Nile; *chiefly from the authority of the Ancients.* By J. Dudley, M.A.

An Itinerary of the Rhone, including part of the Southern Coast of France. By J. Hughes, Esq. A.M. of Oriel College, Oxford.

Manual of Mineralogy. By Prof. Jamieson of Edinburgh. 8vo.

Flora Scotica; or A Description of the Plants indigenous to Scotland and the Isles. By Prof. Hooker of Glasgow. 1 Vol. 8vo.

A Practical Treatise on Diseases of the Heart. By Henry Reeder, M.D. Extraordinary Member of the Royal Medical Society of Edinburgh, and Member of the Medical and Chirurgical Society of London. In which is comprised a full account of all the diseases of that organ, as the Inflammatory, Organic, and Sympathetic, together with their appropriate modes of treatment; also an account of Malconformations of the Heart, Aneurism of the Aorta, Pulsation in Epigastris, &c.

J. B. Benwell intends shortly to publish an Essay on Interest Annuities, chiefly on deducing the values, when payable by instalments at intervals periodically fractional to yearly; as half yearly, quarterly, monthly: with Notes and Illustrations, and a brief Introduction on the study and practice of Life Assurance.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 18. Inquiries relative to the urinary organs and secretion of two species of Rana, common in Ceylon, by Dr. Davy, were read. From the statements of the author it appears, that the bladder of the bull-frog and brown toad (the two species in question) is a genuine receptacle for urine, which it receives from the cloaca in which the ureters terminate; and that their urine is not at all analogous to that of other animals of the order of amphibia, being very dilute, containing urea and certain salts, but no appreciable quantity of lithic acid. This is the more remarkable, as the favourite food of these animals is the same as that of small lizards, whose urine is of a butyraceous consistence, and nearly

pure

pure lithic acid. Hence it appears that the nature of urine, in every instance, depends much more on the peculiar action and structure of the secreting organs than on the peculiarities of diet, or of the circulating fluids.

A paper by Captain Kater was read at the same meeting, containing "An account of the comparison of various British Standards of Linear Measure."

25. "An account of a Micrometer made of Rock Crystal," by Mr. G. Dollond, was read. This contrivance consists in making a sphere of rock crystal, and applying it in place of the usual eye-glass of a telescope, and from its double refracting property, rendering it useful as a micrometer.

Feb. 1. A paper was read on the best kind of steel and form of a compass needle, by Captain Henry Kater.

8. An interesting paper by Captain Kater was read, on the subject of a volcano which he has discovered in the moon. On examining the dark part of the moon through a telescope, he perceived a bright spot resembling a star; and subsequent observations convinced him that it was a volcano. When the part of the moon where it was visible became enlightened, the volcano was no longer visible. In a subsequent observation it may, perhaps, again become visible.

The following papers were also read at the same meeting.

Observations on the Eclipse of the Sun, 7th September, 1820, made at Oxford by Professor Robertson.

A further account of Fossil Bones discovered in caverns in the Limestone Rock of Plymouth, by John Whidby, Esq.

ASTRONOMICAL SOCIETY OF LONDON.

Feb. 9. The *first* anniversary of the above society was held this day, to receive the Report of the Council, and to choose the Officers for the ensuing year. The report was a long one, since it embraced all the points to which the attention of the Council had been directed during the preceding year: and we hope to be able to give a copy of it in our next number, as we conceive it will be highly interesting to all our astronomical readers. The officers elected for the ensuing year are as follows: viz.—

President.

Sir Wm. Herschel, LL.D. F.R.S.

Vice-Presidents.

H. T. Colebrooke, Esq. F.R.S. & L.S.
S. Groombridge, Esq. F.R.S.
D. Moore, Esq. F.R.S. S.A. & L.S.
J. Pond, Esq. *Astronomer Royal*, F.R.S.

Treasurer.

Rev. W. Pearson, LL.D. F.R.S.

Secretaries.

C. Babbage, Esq. M.A. F.R.S. L. & E.
F. Baily, Esq. F.R.S. & L.S.
J. F. W. Herschel, Esq. M.A. F.R.S. L. & E. (*Foreign.*)

Council.

Capt. T. Colby, *Roy. Eng.* LL.D. F.R.S. L. & E.
Sir H. C. Englefield, Bart. F.R.S. L. & E. F.S.A. & L.S.
Davies Gilbert, Esq. V.P.R.S. & F.L.S.
B. Gompertz, Esq. F.R.S.
O. G. Gregory, LL.D.
J. Rennie, Esq. F.R.S. L. & E. S.A. & L.S.
J. South, Esq. F.R.S.
E. Troughton, Esq. F.R.S.

The Names, under each Office, are arranged alphabetically.

WHITEHAVEN PHILOSOPHICAL SOCIETY.

At the last anniversary of this Society, two specimens of meat cured with the pyroligneous acid were exhibited. They were prepared on the 7th of September, 1819. One was hung up at home, and the other sent out to the West Indies, to try the effect of climate upon it, and brought back on the return of the ship to that port. Both specimens were pronounced by all present who tasted them, to be perfectly fresh, sweet, and fit for use, after a lapse of 15 months.

HAERLEM PHILOSOPHICAL SOCIETY.

This Society has proposed the following prize questions: memoirs in answer to be received till 1st January 1822.

"What is known respecting the nature, habits and productions of those little insects which prove so injurious to plants cultivated in hot-houses? and what method does this knowledge suggest for preventing their propagation, or for their extirpation?"

"As large hot-houses are now heated by steam in England, might not our small hot-houses be heated by the same means; and what would be the best apparatus for this purpose?"

"Has it been clearly proved by experience, that there are certain trees and plants, particularly of the most useful kinds, which cannot vegetate close by each other? What experiments can be adduced as proofs of this? Can this antipathy be accounted for by what we know of the nature of plants? and what useful information does it afford for the cultivation of trees and useful plants?"

"What insects are most injurious to trees and shrubs in forests? In what do their injuries consist? What are the proper remedies to prevent such injuries, or to remedy them?"

"What is known respecting the economy of moles? and what means

means does it suggest as most efficacious for freeing lands of them where they prove destructive? On the other hand, are there any observations tending to prove that they are ever useful by destroying other vermin? and how may it be known when moles should be tolerated?"

"Dried yeast having been substituted for wet in brewing, the Society requires, 1. A comparison, founded on chemical analysis, of the nature of yeast both in the wet and dry forms; and a statement of their relative qualities: 2. A method to free wet yeast from the bitter flavour caused by the hop used in brewing: 3. Means by which wet yeast may be preserved, at least for some time, so as to retain the power of fermenting dough."

"As various plants of rapid growth produce a kind of peat, the Society wishes to obtain a succinct and precise statement of whatever has been described or may be observed on this subject: likewise to have it discussed what methods should be followed to promote the growth of some species of peat."

XXII. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

DESTRUCTION OF ZANTE BY AN EARTHQUAKE.

R. M. Academy, Woolwich, 19th February 1821.

SIR,—ALTHOUGH it is past the usual time at which you receive communications for your valuable journal, yet I think you may deem the following account of the late destructive Earthquake at Zante of sufficient interest to give it a place this month. It is extracted from a letter from a relation at Corfu, and I can rely on the correctness of the information it affords. The atmospheric phænomena, which accompanied this earthquake, appear to have been similar to those observed on other occasions, and clearly point out an intimate connexion between the convulsions beneath the surface of the earth and those in the atmosphere, extending even to the higher regions. The circumstances of personal danger, generally attending these convulsions, are such as not to allow of any but the most remarkable phænomena being observed; which is the more to be regretted, as a series of correct observations detailing all the accompanying circumstances would in this, as in every other case, afford the most certain means of arriving at the true explanation of their cause, and perhaps of escaping their most fatal effects. We have at present Englishmen of education and science in nearly every part of the globe: what a service would they render to science, were they in all cases, as much as in their power, to note the *circumstances* attending the phænomena which may fall under their observation, in order that

that they might be published when of sufficient interest! Perhaps an appeal from yourself would tend much to attain so desirable an object. I am, Sir, your obedient servant,

S. H. Christie.

Extract of a Letter dated Corfu, January 10th 1821.

"I have only one piece of news to give you, which is a very sad one, and which perhaps you will have heard before this letter reaches you,—the destruction of Zante by an earthquake. On the twenty-ninth, at three in the morning, we had a very smart shock here, which however did no damage, and, as they are very frequent, little notice was taken of it; but the first boats from Zante brought up news, that on the same morning, at about twenty minutes before four, they experienced a most violent shock, which overthrew a great many houses. The people all ran into the streets, and most into the church of St. Dionysius, the patron saint; but scarce had they been there a short time when another shock, much stronger than the first, came and finished what the other had left undone. Upwards of a thousand houses are much injured; five hundred, or more, so much so that they are obliged to pull them down; and numbers totally overthrown. Providentially very few lives were lost, only ten; but many people severely wounded and bruised. The escape of the officers of the 36th regiment was most miraculous. They had given a great party to the gentlemen of Zante, and broke up only ten minutes before the earthquake; the house was thrown down to its very foundation. The house of Cavalier Bulga, the finest in the town, is also destroyed, together with a most valuable library.

"What rendered the state of these unfortunate people much more lamentable, was that, immediately after, a most violent hurricane arose, accompanied by such a shower of hail as has never before been experienced. You must not accuse me of exaggeration, for I copy our official dispatches, when I tell you that the hailstones were an inch and a half in diameter, and weighed nearly three ounces. This shower lasted some time, and then changed to violent torrents of rain: two people were drowned in the streets: and to complete all, the sea rose and carried away two more houses. The house of Sir Patrick Ross the governor is half down. Lord Strangford (the English ambassador) and his Lady, notwithstanding the hurricane, were obliged to go on board the Cambrian frigate, which when the dispatches came away was not expected to ride out the gale.

"Only conceive the misery of these poor people, who, afraid to return into their houses, and many without any to return to, were exposed three days and nights in the open air to such dreadful

ful weather. What further damage may have been done is not known, or whether they have had any more shocks, as it has blown so tremendously for this last fortnight that no boats have been able to put to sea. Great fears are entertained that they have had another, as we last week felt two very smart shocks here, one at half past seven in the evening and another at one in the morning. This earthquake has been felt in all the Islands and at Malta, and we are afraid Sicily has suffered. This is the second town which has been destroyed in two years. Zante was the largest and best built town of all the Islands, and would have been reckoned a fine town on the continent. As to Santa Maura, it is a most wretched place. The earthquakes there lasted two months, and during that time there were felt eight hundred and thirty shocks great and small. Every house in the town and castle is cracked in all directions, and more than half thrown down, the barracks destroyed, and the bridge broken in many places. One shock in particular was very remarkable, for the earth trembled or shivered continually from half past five in the afternoon till twelve at night.

"You cannot conceive the extraordinary and fearful feeling that an earthquake produces, nor is it possible to describe it; it is sufficient to say they are phænomena that, let them be ever so frequent, I never shall become indifferent to, but shall always make a run to get under a door or a window, which is the only chance of safety in case the roof falls in. We were in the habit of complaining of the water coming through the roof in every part: we are not quite so nice now; and so as the roof itself will have the goodness to keep its own place, we think ourselves most fortunate."

Jan. 14.—"The courier is on the point of sailing; I therefore can only say a very few words. The earthquake at Zante has been still more calamitous than was known; four hundred and sixty-three houses have been totally destroyed, and five hundred more so much injured that it will be necessary to pull them down, —beside whole villages in the country totally destroyed."

STATISTICS.

To Mr. Tilloch.

Dear Sir,—In consequence of several reports made to me respecting the extreme longevity of some Russian subjects, I wrote to a friend at Petersburg, and received the inclosed official statement for the year 1817. If you consider this document worth preservation, be so good as to print it in your journal.

Dear Sir, your much obliged servant,

Soho Square, January 18, 1821. Ant. Carlisle.

In

In all Russia, in the year 1817.

Born 786,810 male
711,796 female } 1,498,606, or 41,000 more than in 1816.
Died 423,092 male
405,469 female } 828,561, or 8178 more than in 1816.

Of which, under 5 years 208,954 !

Died, males of more than				60	years of age			68,723
..	..	..	..	70	..	..		38,764
..	..	..	..	80	..	..		16,175
..	..	..	..	90	..	..		2,108
..	..	..	..	100	..	..		783
..	..	..	..	115	..	..		83
..	..	..	..	120	..	..		51
..	..	..	..	125	..	..		21
..	..	..	..	130	..	..		7
..	..	..	..	135	..	..		1
..	..	..	..	140	..	..		1

Of females, no return.

Greatest number of deaths (children excepted) between 60 and 65 years.

Born 670,045 more than died.

Married 339,069 or 9836, more than in 1816.

At 60	to 70	years every		(circa)		6th man
70	80	..	..	..	11	
80	90	..	..	..	26	
90	100	..	..	..	207	
100	115	..	..	..	540	
115	120	..	..	..	5,097	
120	125	..	..	..	8,296	
125	130	..	..	..	20,147	
130	135	..	..	..	60,442	
more than	135	..	..	..	423,092	
	140	..	..	..	423,092	

France by the last census, and by documents furnished by the Board of Statistics, contains 29,217,405 souls. Births in Paris in 1819—24,344, of which 8,641 were natural children: deaths 22,072, including 351 children who died of the small pox: still-born children 1,352: marriages 6,236. Population of Paris 713,765.

COMPRESSIBILITY OF WATER.

In our last we gave an account of Mr. Perkins's experiments on this subject, in which he states the compression effected by a pressure of 100 atmospheres to be "*about one per cent.*;" but Dr. Roget, having calculated the degree of compression produced by

by the piezometer in the first experiment from the data furnished by Mr. Perkins, has discovered a material error in his computation. Dr. Roget (in the Annals of Philosophy No. 2) states that the real compression was in fact only $\frac{1}{212.38}$, or a little less than *one half per cent.*—a result which agrees very nearly with that of Canton. As deduced from Canton's experiments the height of the modulus of elasticity is 750,000 feet*, while those of Mr. Perkins when correctly computed by Dr. Young's method is shown to be 743,260 feet; the difference being less than one hundredth of the whole. So near an agreement in experiments conducted by different methods, is very satisfactory, and bears the strongest testimony in favour of the accuracy of those of Mr. Perkins, in as much as he himself was not aware of that agreement.

THE UNICORN DISCOVERED.

Major Latter, commanding in the Rajah of Sikkim's territories in the hilly country east of Nepaul, has addressed to Adjutant-General Nicol a letter, in which he states that the Unicorn, so long considered as a fabulous animal, actually exists at this moment in the interior of Thibet, where it is well known to the inhabitants. "This," says the major, "is a very curious fact, and it may be necessary to mention how the circumstance became known to me. In a Thibetian manuscript containing the names of different animals which I procured the other day from the hills, the Unicorn is classed under the head of those whose hoofs are divided: it is called the one-horned *tso'po*. Upon inquiring what kind of animal it was, to our astonishment, the person who brought me the manuscript described exactly the Unicorn of the ancients: saying, that it was a native of the interior of Thibet, about the size of a *tattoo* (a horse from twelve to thirteen hands high), fierce and extremely wild; seldom, if ever, caught alive, but frequently shot; and that the flesh was used for food. The person who gave me the information has repeatedly seen these animals, and eaten the flesh of them. They go together in herds like our wild buffaloes, and are very frequently to be met with on the borders of the great desert, about a month's journey from Lassa, in that part of the country inhabited by the wandering Tartars."—This communication is accompanied by a drawing made by the messenger from recollection: it bears some resemblance to a horse, but has cloven hoofs, a long curved horn growing out of the forehead, and a boar-haped tail, like that of the "*fera monoceros,*" described by Pliny. From its herding together, as the unicorn of the scriptures is said to do, as well as from the rest of the description, it is evident that it cannot

* Young's Lectures on Natural Philosophy, i. p. 276.

be the rhinoceros, which is solitary animal; besides, Major Latter states that, in the Thibetian manuscript, the rhinoceros is described under the name of *servo*, and classed with the elephant; "neither," says he, "is it the wild horse (well known in Thibet), for that has also a different name, and is classed in the MS. with the animals which have the hoofs undivided."—"I have written (he subjoined) to the Sachia Lama, requesting him to procure me a perfect skin of the animal, with the head, horn, and hoofs; but it will be a long time before I can get it down, for they are not to be met with nearer than a month's journey from Lassa."—*Quarterly Review.*

ASTRONOMICAL INSTRUMENTS.

The Chevalier Theodore Carezzini, a Piedmontese, has invented two kinds of round tables, which he calls geocentric and heliocentric tables, and by their aid, a person without any knowledge of mathematics can, in a very short time, thoroughly observe the course of the stars, and explain the celestial phænomena. Ladies, and youths, whom the inventor has instructed in his method, have, without much previous knowledge of astronomy, solved various problems respecting the sun, the moon, the planets, fixed stars, eclipses, &c. By means of these instruments, you may, in the open air, obtain a meridian line in a few minutes: and in a journey by land, never miss the direction to the north. You may also learn the hour during the night, without a watch. It is remarkable, that in the country the geocentric table may appear in the shape of an astronomical garden, of whatever size you please. It is to be hoped that the inventor of this new method, of which we have given this imperfect notice, will be able to overcome all the difficulties which usually oppose useful inventions of this kind.—*From a German Journal.*

ESSENTIAL OIL OF TAR LIGHTS.

In a considerable district of the town lying on the west side of Tottenham Court Road, a very improved kind of street-lamps have been introduced, which in the whiteness and intensity of their lights, far exceed the street gas lights, under the same bulks of flame;—each of these lamps being independent, there is no danger of a whole district being left in darkness at once, through an accidental or designed stoppage or destruction of the Gas Main, nor are the inhabitants burthened by any expense of service-pipes, and the many *et ceteras* of that mode of lighting. On inquiry we have learned, that these lamps have been supplied by the honourable Major Cochrane, under patents to his brother Lord

Lord Cochrane, and to himself: the former being, for the mode of distilling and managing the oil, and the latter for the construction of a lamp, calculated for burning this pellucid and very volatile and inflammable oil, closely resembling if not identically the same with purified *naphtha:*—which oil, in its greatest perfection, is prepared in Scotland, at once from the coals, as is said, by a relative of Mr. Cochrane's: the essential oil prepared from the Gas-work Tar, is found, whenever the wicks of the lamps are trimmed the least too high, to deposit carbon on the wicks, which the Scotch oil never does, and in such case to occasion lamps supplied therewith, to smoke, and sometimes, owing to the very great volatility of this Gas-work oil, a smoking lamp has been filled with explosive vapour, which has taken fire and destroyed the lamp-glass: accidents which have never happened with the use of the Scotch oil. From the facility of preparing this oil at any colliery and in any quantity, and from the cheapness of its conveyance to Town by the canals, we anticipate, that this mode of lighting our streets and roads will, ere long, become very general.

EARTHQUAKE.

On Christmas night, a smart shock of an earthquake was felt along the west coast of Kintail and about Loch Hourn and the intermediate places. It was also felt about the heights of Glenmoriston, and in other central places of the county.

GERMAN SAUSAGES.

Dr. J. Kerner has discovered that smoked sausages, a favourite food of the inhabitants of Wirtemberg, contain often a deadly poison. The effects of the poison are ordinarily manifested in spring time or the month of April, in a manner more or less alarming. In a periodical paper which appears at Tubingen, Mr. Kerner has published a number of observations on the subject, and he has now in the press, a work in which he treats of it more in detail. He states that, out of 76 persons who fell sick from having eaten sausages, 37 died in a short time; while others remained valetudinarians for years. Liver sausages appear to be the most dangerous. In general (says M. Kerner) the poison is formed in raw, hashed, and seasoned flesh, after being stuffed in gut and smoked. This animal poison is distinguished from all others by this circumstance—that it does not attack the brain and spinal marrow, while it paralyses the whole lymphatic system. Sometimes the patient for many

months

months together ceases to feel his heart beat, whilst the pulsation of the arteries remains invariable. All the observations of M. Kerner are supported by cases which have come within his own experience.

AGRICULTURE.

Last year Mr. Falla, of Gateshead, Northumberland, transplanted from a seed-bed to a piece of land worked by the spade, wheat which he ranged into rows six inches apart: this yielded seventeen coombs per acre. Another piece of the land on which the rows were planted twelve inches apart, yielded fifteen coombs. Another piece sown in drill, and a fourth in broad-cast, yielded nineteen coombs per acre. The produce of this land by ploughing is usually about six coombs.

A field of seven acres, in the county of Surrey, was prepared last year by the spade for barley. The labourers earned in the winter at the rate of 15*s.* per week, two-pence per rod being paid for digging; and the proprietor believes it would have cost him double the expense if he had ploughed it.

IODINE IN SPONGE.

M. Straub has ascertained to a certainty, that iodine exists in sponge. Burnt sponge was washed with water, the solution was decomposed by sulphuric acid, and enough of iodine was obtained to confirm the idea that the medicinal properties ascribed to the sponge, are owing to the presence of this substance. He therefore recommended that preparations of iodine should be tried in medicine, or an alcoholic extract from burnt sponge, in preference to the burnt sponge itself.—He has also detected iodine in turf.—*Bib. Univ.* xiv.

LIST OF PATENTS FOR NEW INVENTIONS.

To James Ferguson Cole, Chelsea, watch and chronometer maker, for certain improvements in chronometers.—Dated the 27th of January 1821.—2 months allowed to enrol Specification.

To John Roger Arnold, of Chigwell, chronometer and time-piece maker, for his new or improved expansion balance for a chronometer.—27th Jan.—2 months.

To Alphonso Doxat, of Bishopsgate-street, esq., in consequence of a communication made to him by a certain foreigner, of a new combination of mechanical powers, whereby the weight and muscular force of men may be employed to actuate machinery for raising water or other purposes in a more advantageous manner than has been hitherto practised.—27th Jan.—6 mo.

To Phillips London the younger, of Cannon-street, practical chemist,

chemist, for certain improvements in the application of heat to coppers and other utensils.—3d Feb.—6 months.

To William Aldersay, of Homerton, for improvements on steam-engines and other machinery where the crank is used.—3d Feb.—4 months.

To George Vizard, of Dursley, clothier, for a new process or method of dressing and polishing goods of woollen manufacture.—3rd Feb.—2 months.

To Thomas Masterman, of No. 38, Broad-street Ratcliffe, brewer, for certain machinery for the purpose of imparting motion to be worked by steam and water, without either cylinder or piston, and with less loss of power than occurs in working any of the steam-engines now in use.—10th Feb.—4 months.

To Robert Stein, of No. 7, Walcot-place, Lambeth, brewer, for certain improvements in steam-engines.—20th Feb.—6 mo.

To James Foster, of Stourbridge, iron-master, for certain improvements in the manufacture of wrought malleable iron.—20th Feb.—6 months.

ON THE SOLAR SPOTS.

SIR,—In the Phil. Mag. for last month (Jan.) page 67, were inserted some observations (signed "A CORRESPONDENT) on a paper which four years since (vol. xlix. p. 182) was communicated to that publication at the desire of a friend of yours, but drawn up by me. The object of the essay was to prove, that an opinion, which was universally prevalent, respecting the influence of the solar spots, on the seasons, was entirely without foundation;—and to show, that during the inclement year 1816, those spots were neither unusually large, nor numerous; so that the obscuration never occupied any considerable portion of the sun's disc.

Under these circumstances, the direction of the solar axis, in the diagrams, which this "*correspondent*" perceives to be erroneous, was never intended to be given with accuracy; as it was a point of no material consequence to the inquiry. Why it was placed at 8° west, I cannot now explain, except that it was not from supposing that, viewed from the earth, it constantly retained that position, in all parts of the orbit. Such a supposition would have been absurd. It was convenient to place the axis *somewhere*; and if it was within the greatest elongation of the pole from the vertex, it was sufficiently true for the purpose intended.

The first fig. pl. III, represented two spots, the largest which had been observed during several months, on the sun's disc; and was designed to show what proportion of the area they seemed to cover. Fig. 3. represented the situations of a spot on Nov. 1st and 2d; and was designed to show that its motion in 24 hours,

was

was through more than one fourth of the sun's diameter. This was a remarkable phenomenon, and I have every reason to believe, no deception arose from inaccuracy in taking the positions. Fig. 4. showed the places and dimensions of 15 spots, visible on Oct. 8th, but all of which disappeared before 11th. Now it is obvious that in these delineations, it was perfectly indifferent in what direction the axis of the sun lay. In Fig. 2, however, it would have been better to have constructed the true projection. The place of the sun's pole and nodes might have been deduced from 3 positions of the spot, with the chords of the intermediate arcs, and the zenith distances, by Cagnoli's problem; or by a simple process, having the nodes given. This was deemed unnecessary, as it was evident from inspection, that the path of a spot, passing near the centre, and over the limb at an angle of about 45° above the horizontal line, could not have moved parallel with the equator, even supposing the earth in that part of its orbit when the equatorial diameter is most oblique to view. As this deviation therefore was unusual, it was observed in the sentence immediately following, that "in general the course of the maculæ is, with little variation, parallel to the sun's equator; but the direction in which these proceded, was very singular and curious. The passage from Adams's Lectures was then quoted to sanction this remark. Adams's work is not at hand, but I conceived that the sentence quoted, referred exclusively to the *real* motion of the spots, and not to the *apparent* motions occasioned by the revolution of the earth in its orbit; nor to diurnal parallax. My observàtions were all made within a few minutes of the meridian, and therefore diurnal parallax was out of the question. It was too much to infer that the effects of the earth's annual motion, and diurnal parallax, were unknown to me; and therefore the concluding sarcasm was *unnecessary*. I may be allowed, perhaps, to return the compliment; for it is amusing to observe your "*Correspondent*" calling the *luminous* spots sometimes seen on the disc fæculæ! Such a mistake, I may retort in his phrase applied to me, "must be the result of a very slender acquaintance with the subject." Faculæ and Fæculæ differ as much as the flame at one end of a torch does, from the pitch at the opposite end; or, in other words, as light and darkness.

It is always right to take leave in good humour; and I doubt not that a Latin dictionary will enable this "*Correspondent*" to laugh at his own error as much as I have done.

Your obedient servant,

Feb. 10, 1821. W. M. Moseley.

To Mr. Tilloch.

MERCURIAL ATMOSPHERE.

Mr. Faraday states (in the Quarterly Journal of Science) that a small piece of leaf-gold attached to the under part of the stopper of a bottle, the bottom of which was covered, about an eighth of an inch deep, with mercury, having been set aside in a dark and cool place for six or eight weeks, was found at the end of that time whitened by the mercury. This experiment has been repeated several times, care being taken that mercury should not get to the gold, but by passing through the air in the bottle, and always with the same result. It has long been admitted, that in the upper part of the barometer and thermometer there is an atmosphere of mercury, even in common temperature; and now it is proved that even in the air mercury is always surrounded by an atmosphere of the same substance.

NEW COMET.

We announced in our last, that a new comet had been seen at the Royal Observatory, Paris, on the 21st of December. We now learn that the same comet was discovered on the same night about seven o'clock by Signor Pons, astronomer of the Duchy of Lucca. It appears like a white spot, not thick nor of a kernel form, and with a very small tail. On the 22d, Signor Pons continued his observations, and perceived that the comet had not changed its position, but that its tail was becoming more visible, and its light had acquired greater intensity. He thence concluded that this comet will become rapidly more luminous, and to such a degree as to be visible by the naked eye. Astronomers have fixed its right ascension at 0 deg. 30 min., and its northern declination at 18 deg. between the stars of the sixth magnitude ξ and υ of *Pegasus*.

ASTRONOMY.

Sir,—I have been very anxious to meet with a variety of good observations of the late solar eclipse, in order to determine the error of the lunar tables. From the observations of Mr. Evans, inserted in your Number for December, it appears that the tables of M. Burckhardt came nearer the truth, on this occasion, than those of M. Burg.

In calculating the elements from both these tables, I found the greatest differences in the moon's semidiameter and horizontal parallax. These two elements, taken from Burckhardt, unite in making the instant of commencement later than it comes out by Burg's tables; but, for the end, their effects nearly counterbalance one another, so that the instant of last contact comes

out

out nearly the same by both tables. From this I had a suspicion, that the error of the tables of Burckhardt in longitude is less than in these two other elements, which I think has been so far confirmed by Mr. Evans's observations.

I should be glad if some of your correspondents would give you, for publication, the diameter of the moon as measured on the sun's disc. The apparent diameter, at the instant of greatest observation for Greenwich, is 29′ 38·″98 by Burckhardt's tables, and 29′ 43·″16 from those of Burg.

I am, dear sir, your most obedient servant,

To Mr. Tilloch. GEORGE INNES.

A REPLY TO MR. INNES.

(See our last Volume, page 436.)

SIR,—I am much obliged to your correspondent, Mr. Innes, for suggesting the probability of an error in the time of my observation of the solar eclipse on the 7th of September last. I have examined the minutes made on that day and the calculations, and find I had properly applied the equation of time, but I had copied the reduced time *for Greenwich*, instead of that for this place. The longitude of this place being 2^m 39^s west of Greenwich, this time should be subtracted from the time before given, and will stand thus:

Mean time at Leighton $\begin{cases} \text{begin} & 12 \quad 18 \quad 46 \\ \text{end} & 3 \quad 10 \quad 28. \end{cases}$

I beg also to state, that the time was obtained from six altitudes of the sun on the morning of the 7th of September, three of upper and three of lower limb giving the mean altitude 17° 6′ 22″ time by chron. 7^h 5^m 26^s.

Barom. 29·86 } from which I made the error of chrono-
Therm. 50° } meter 12^m $26\frac{1}{2}^s$.

To Mr. Tilloch. B. BEVAN.

METEOROLOGY.

Croom's Hill, Greenwich, Dec. 31, 1820.

SIR,—I have the pleasure of handing to you an account of the quantity of rain fallen, and the quantity of evaporation that has taken place at Greenwich in Kent, during the year 1820. I have subjoined the whole quantity for the last four years, and you will perceive that the register of the past year has been less than any other, and very greatly less than that of 1819.—In the midland counties inconvenience has been felt for the want of rain, as the mills in many, and the canals in several instances have stopped working for the want of water.

Ever, sir, yours,

To Mr. Tilloch. HENRY LAWSON.

	Rain.			Evaporation.
1817	.. 25·349	..	..	22·227
1818	.. 24·252	..	..	27·064
1819	.. 27·339	..	..	21·369
1820	.. 23·274	..	..	19·621

Months.	Rain.	Evaporation.	Months.	Rain.	Evaporation.
From 26 Dec. 1819, to 2 Jan. 1820,	0·167	0·031	1820. 9 to 16 July.	0·663	0·389
2 to 9	0·116	0·	16 to 23	1·407	0 620
9 to 16	0·113	0·	23 to 30	0·018	0·844
16 to 23	1·478	0·	30 to 6 Aug.	1·963	1·909
23 to 30	0·359	0 124	6 to 13	0·069	0·824
30 to 6 Feb.	0·005	0·086	13 to 20	0 062·	0·751
6 to 13	0·168	0·122	20 to 27	·233	0·507
13 to 20	0·596	0·055	27 to 3 Sept.	0·171	0·624
20 to 27	0·150	0·061	3 to 10	0·004	0·508
27 to 5 Mar.	0·134	0·	10 to 17	0·026	0·568
5 to 12	0·031	0·301	17 to 24	1·937	0 354
12 to 19	0·002	0·346	24 to 1 Oct.	0·282	0·284
19 to 26	0·064	0·411	1 to 8	0 023	0 311
26 to 2 April.	0·076	0·423	8 to 15	0 207	0·185
2 to 9	0·337	0·435	15 to 22	1·227	0·293
9 to 16	0·825	0·204	22 to 29	0·770	0·185
16 to 23	0·001	0·773	29 to 5 Nov	0·091	0·
23 to 30	0·256	0·713	5 to 12	0·221	0·100
30 to 7 May.	0·055	0·637	12 to 19	0·608	0·021
7 to 14	0·023	0·761	19 to 26	0·650	0·064
14 to 21	1·636	0·841	26 to 3 Dec.	0·030	0·064
21 to 28	1·000	0·774	3 to 10	0·115	0 175
28 to 4 June.	0·706	0·635	10 to 17	1·240	0·068
4 to 11	0 352	0·417	17 to 24	0·169	0·053
11 to 18	0·585	0·386	24 to 31	0· 005	0·023
18 to 25	0·568	0·798	Inches.	23·274	19·621
25 to 2 July.	0·068	1·000			
2 to 9	0·192	0·472			

Crumpsall, Lancashire, Feb. 12, 1821.

Sir,—In compliance with the wishes of Mr. Bevan, I send you my observations on the barometer, &c. for the 12th of the present month.

1821.	A. M.	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
Feb. 12th	8h.	29.920	35.5	29.5	Calm.	Sharp frost with dense fog.
	9	29.910	34	28.5	Do.	Fog thinner, faint sunshine.
	10	29.920	31.5	28 5	Do.	Do.
	11	29.930	31	29	Do.	Fog denser.
	12	29.930	33 5	29.5	Do.	Fog thinner and higher [sunshine,

The place of observation is situated in the township of Crumpsall, about two miles N. by E. from Manchester. The barometer is a common upright one, and the internal diameter of the tube bears so small a proportion to that of the bulb, that the connection is too inconsiderable to merit any attention, being only $\frac{1}{2000}$ part of the whole scale: that is, the scale of four inches should be diminished by $\frac{1}{500}$ of an inch. The surface of the mercury in the bulb is $15\frac{1}{2}$ feet from the ground, and about 248 feet above the level of the river Irwell where it flows under the bridge at the bottom of Bridge Street, Manchester; to which place it is navigated by the flats of the Mersey and Irwell Navigation Company. A mean of four attempts to determine the elevation of the surface of the mercury in the bulb above the bridge barometrically, gives 220 feet; and the height of the bridge above the level of the river measures exactly 28 feet. If, therefore, the elevation of the barometer above the level of the sea (which I have reason to believe may be somewhere about 320 feet) were carefully ascertained, the elevation of the bridge, and of the river below it also above the same level, would be easily determined.

I am &c.

To Mr. Tilloch. JOHN BLACKWALL.

Hafod, Mold, Flintshire, Feb. 13, 1821.

SIR,—I beg leave to transmit to you my observations made yesterday with a barometer of Sir H. Englefield's construction made by T. Jones. The situation I take to be about 500 feet above sea level, but the altitude has not yet been accurately taken. It is 16 miles South West of Liverpool. I am &c.

To Mr. Tilloch. WM. WARD.

Feb. 12, 1821. A. M.	Bar.	Ther. att.	Ther. det.	Wind, &c.
8h.	29.742	38	26	E. Misty and calm.
9	—.748	38	25	——
10	—.753	38.5	26	——
11	—.764	38.5	26	N.E. Do. and slight breeze.
12	—.768	38.5	26	——

SIR,—I should feel much gratified could any of your readers furnish such information as would reconcile the following levels, (which are apparent contradictions of each other) as it would give the fall of the Thames from Brentford to Limehouse, and be a great assistance in forming an estimate of the height of London above the sea.

The

	feet	in.		
The rise of the Grand Junction Canal from the Thames at Brentford, to where the Paddington Canal joins it, is, according to Middleton's Agricultural Report of Middlesex	91	8		
Ditto according to Galton in Annals of Philosophy, Vol. IX. page 179, and art. Canal Rees's Cyllopædia	90	0		
The Paddington Canal is level.				
The bottom of the sewer at the north end of Baker-street below the Paddington Canal	15	$11\frac{1}{2}$		
The fall of the sewer from Baker-street to the level of half flood in the Thames at Northumberland-street, is, according to Mr. Rennie's Report in the First Report of the Commissioners of His Majesty's Woods, Forests, &c. p. 129, and 144, ...	76	6	92	$5\frac{1}{2}$
The fall of the Regent's Canal is stated, in the 56th Vol. of the Philosophical Magazine, page 315, to be	86	0		

It is not mentioned at what state of the tides the rise of the Regent's Canal commences; but the heights on the Grand Junction are said to be from high water mark, and the range of the tide is about 18 feet at Northumberland Street.

For your correspondent Mr. Bevan, I send you the height of the barometer on the 12th instant. The cistern of the barometer is about 30 feet above the level of the Paddington Canal. I am sorry I had not an opportunity of getting more than one observation; but as the day was very steady and favourable, I hope it will not be useless. Your constant reader,

To Mr. Tilloch. T. R.

Feb. 12 1821 10 o'clock morning Bar. 30·185. Ther. att. 36°. Ther. det. 34°.

Arlington-Street, Camden-Town, Feb. 14, 1821.

Sir,—In compliance with the invitation made by Mr. Bevan, in your Journal for December and January last, I send you my observations made in this place on the 12th instant. The nearest water level to my habitation is that of the canal of Paddington, but its relative height I do not yet know.

	Barom.	Ther. att.	Ther. det.	Wind.
Feb. 12. 8h M.	29·955	52·5	40·2	Cloudy. N. E.
12	29·960	55·5	40·2	Ditto.
$5\frac{1}{2}$ E.	29·950	59·	36·	The wind abated.

The detached thermometer in the air was exposed to the N.

Since the invitation of Mr. Bevan to observe, with the purpose of ascertaining the relative level of places, the best opportunities

 afforded

afforded by the barometer, have been in the days of this month, from the 5th to the 8th included; because the barometer has been almost steady between 30.78 to 30.48. But until we can obtain the mean of the heights of the barometer for at least a whole year, we shall have very little chance to ascertain by this instrument, the height of these places above the level of the sea.

To Mr. Tilloch. A CONSTANT READER.

Epping, Feb. 15, 1821.

SIR,—The following barometrical observations made at this place, were taken with the greatest care, and exactly at the times proposed.

1821. Hour. A.M. M.T.	Barom.	Ther. att.	Ther. det.	Wind.	Weather.	
Jan. 8th 8h	28·871	38	38	S.E.	Foggy.	Very little wind the whole time, scarcely perceptible at 8 and 9 o'clock.
9	28·870	38	38	E.S.E.	—	
10	28·870	38	38	E.S.E.	—	
11	28·868	39	39	E.S.E.	—	
12	28·848	39	40	E.	—	
Feb. 12th 8h	29·888	38	30	N.N.E.	Foggy.	
9	29·889	38	32	N.E.	—	
10	29·895	38	33	N.E.	—	
11	29·895	37	34	N.E.	—	
12	29·895	37	34	N.E.	—	

It is much to be hoped, that gentlemen who have good instruments for ascertaining the correct atmospheric pressure, will take the trouble of making very careful observations on the second Monday in every month, at the hours of 8, 9, 10, 11 and 12 in the morning, as previously fixed on by Mr. Bevan.

I am confident such observations will have their use, not only in ascertaining the altitude of different stations above zero, or the surface of the sea at low water, but may probably be the means of leading to some discoveries relative to the pressure of the atmosphere, that are not at present known.

From corresponding barometrical observations made here, and at London Bridge, together with those published in your valuable Journal, I am led to believe that the altitude of Epping, above the surface of the sea, is not quite 400 feet; yet the hill called *Highbeech*, situated about five miles S.W. of Epping, and which is not higher than this place, is stated (from observations made in the course of the Trigonometrical Survey) at 790 feet above the level of the sea!

Yours truly,

To Mr. Tilloch. THOMAS SQUIRE.

Epping,

Epping, Feb. 16, 1821.

Sir,—I have this day received the following barometrical observations, taken on the 12th of this month, at Pocklington, near York, by my friend Mr. Wm. Rogerson, Jun.

Monday, Feb. 12, 1821.		Barom.	Ther. att.	Out door Ther.	Wind.	Weather.
Hs. AM.	8	30·237	42	28.7	N.N.E.	Sharp and frosty: gen-
	9	30·239	41	28.4	N.E.byN.	Do. [tle breezes: sky
	10	30·251	41	28.8	N.E.byN.	Do. [covered with grey
	11	30·254	41	29.3	E.	Do. [misty clouds—
	12	30·255	40	31.3	N. by W.	Do. [thin.

The instruments from which the above observations were made, are of the very best of the kind. The basin of the barometer is about nine feet from the surface of the ground.

The latitude of Pocklington is 53° 56′ N., and its longitude 0° 45′ W. from Greenwich.

Yours most respectfully,
Thomas Squire.

Bristol, Feb. 17, 1821.

Sir,—I herewith transmit you a statement of the height of the mercurial column in this city on the 12th instant. The instrument made use of is an open cistern barometer, situated 76 feet above the rivers Avon and Froome, which being dammed up—one mile below the city—the height from which I measured may be considered the average high-water mark.

To Mr. Tilloch. Edward Jones.

		Barom.	Ther. attached.	Ther. detached.
Feb. 12th	8 AM.	30 09	36	29
	9	·09	36	30
	10	·10	38	31
	11	·10	38	33
	12	·10	39	34½

Leighton, Feb. 21, 1821.

Dear Sir,—I send you the barometrical observations made at this place on Monday the 12th instant; and also those made at Bushy-Heath by Colonel Beaufoy.

LEIGHTON

LEIGHTON.

	Barom.	Ther. att.	Ther. det.	Wind.
8h	29·995	$32\frac{1}{2}$	30	E.N.E.
9	30·000	33	$30\frac{1}{2}$	N.E.
10	30·007	$33\frac{1}{2}$	$30\frac{1}{2}$	N.E.
11	30·017	$33\frac{1}{2}$	31	N.E.
12	30·017	$33\frac{1}{2}$	$32\frac{1}{2}$	E.

BUSHY-HEATH.

	Barom.	Ther. att.	Ther. det.	Wind.	Weather.
8h	29·751	36	$30\frac{1}{2}$	N.E. by E. moder.	Foggy.
9	29·751	36	31	N.E. by E. do.	do.
10	29·755	36	32	N.E. by E. fresh.	do.
11	29·760	36	$32\frac{1}{2}$	N.E. by E. do.	Cloudy.
12	29·756	36	35	N.E. by E. do	Hail.

From some calculations already made, it appears that Colonel Beaufoy's barometer is about 230 feet above mine, and probably about 526 above *high water* in the River Thames at Somerset-House; but how much the point of high water at that place is above low water spring tides at open sea, remains a desideratum not much to the credit of our great metropolis, and which it is to be hoped will soon be removed.

I beg leave in this place to correct a note published in page 18 of your last Number, by Mr. Cary, relative to the estimated height of his instrument above the *mean* level of the sea, as that calculation was founded upon a reported height of the Royal Society's barometer, which I afterwards found, by levelling, not to be quite correct.

There is also at present an uncertainty about the *mean level* of the sea, depending upon the extremes of high and low water at both spring and neap tides. But until some more correct determination of the relative height of the range of the tides at sea, to some fixed spot in London, is made, it will not be prudent to assume a quantity that may soon require correction; instead, therefore, of 71·25 feet above the *mean* level of the sea, I would have it read 56·72 feet above *high water* in the *Thames* near Somerset-House.

From the very valuable instruments used in the Trigonometrical Survey of Great Britain, I was induced to expect some accurate determinations of the heights of the principal stations selected on that survey. I have had opportunities of trying the relative height of five of these stations, and am sorry to find a variation amongst them of more than 50 feet. B. BEVAN.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
Jan. 15	11	35·5	29·85	Cloudy—heavy rain P.M.
16	12	48·	29·55	Stormy
17	13	45·	29·80	Cloudy
18	full	50·	29·95	Ditto
19	15	49·	30·05	Ditto
20	16	49·	30·10	Fine
21	17	41·5	30·50	Ditto
22	18	47·	30·45	Cloudy
23	19	39·	30·65	Fine
24	20	34·	30·54	Cloudy
25	21	44·	30·45	Ditto
26	22	37·	30·44	Ditto
27	23	33·5	30·23	Ditto
28	24	34·5	30·15	Ditto
29	25	33·5	30·05	Fine
30	26	48·	30·05	Ditto
31	27	49·	30·05	Ditto
Feb. 1	28	53·5	30·10	Cloudy
2	new	47·5	30·	Fine
3	1	48·	30·	Cloudy
4	2	45·5	29·80	Fine
5	3	39·	30·46	Ditto
6	4	40·	30·60	Cloudy
7	5	42·5	30·45	Fine
8	6	46·	30·33	Ditto
9	[illegible]	46·	30·05	Ditto
10	8	43·5	30·33	Ditto
11	9	37·	30·33	Cloudy
12	10	40·	30·20	Fine
13	11	39·5	30·20	Ditto
14	12	36·	30·15	Cloudy

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For February 1821.

Days of Month. 1821.	Thermometer 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom Inches.	Weather.
Jan. 27	[illegible]	37	36	30.45	Cloudy
28	35	35	33	·30	Cloudy
29	32	41	33	·26	Fair
30	33	48	45	·36	Fair
31	47	50	47	·48	Cloudy
Feb. 1	47	52	49	·45	Fair
2	49	50	40	·40	Fair
3	35	49	41	·39	Fair
4	40	47	35	·04	Small fall of rain.
5	30	39	33	·81	Fair
6	31	42	32	·85	Fair
7	29	42	36	·75	Fair
8	35	49	38	·62	Fair
9	30	46	35	·20	Fair
10	40	45	36	·37	Fair
11	33	40	32	·34	Fair
12	30	39	33	·29	Fair
13	33	38	32	·33	Cloudy
14	32	35	32	·30	Cloudy
15	32	36	33	·44	Cloudy
16	32	36	32	·57	Cloudy
17	30	32	32	·37	Cloudy
18	31	37	37	·18	Cloudy
19	30	39	30	·36	Fair
20	25	38	38	·16	Cloudy
21	25	43	37	·24	Fair
22	35	40	30	·34	Fair
23	28	40	30	·30	Fair
24	28	36		·18	Foggy

N B The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
10th February 8 o'Clock M Barom 30 340 Ther. attached 16° Detached 40
— — — 9 — — — — ·346 — — — 47 — — 42
— — — 10 — — — — ·360 — — — 47 — — 42
— — — 1 — — — — .372 — — — 48 — — 4[illegible]

XXIII. *New analytic Formula and Table of an increasing Life Annuity; with Remarks on the Surrender of Life Assurance Policies at Proprietary Offices.* By Mr. J. B. Benwell, *Hoxton.*

To Mr. Tilloch.

Sir, — The accompanying table, computed from real observations and the probabilities of life, is applicable to a description and species of life annuity transaction, by no means common, and scarcely, under any form whatever, practised in this country; and any thing new or novel of this kind I am inclined to imagine will be interesting to, and invite the attention of societies engaged and concerned in this particular and important branch of human œconomy. On a former occasion I recommended the subject to their notice; and I do it now with the same persuasion of its being, by an easy extension of application, as suitably adapted to the views and purposes of individuals, as any other mode of assurance, and of its becoming at some future period in more general recurrence. I do not expect any credit for the suggestion proposed, because we know that, in the commerce of life, according as interest and gain may impart a colouring to the medium an object is seen through, it is either abandoned, or pursued with avidity. Now it happens in the instance under review, that the influence of this ordinary spirit and idol of general adoration is to be perceived: for the Offices (I speak in reference to the companies constituted solely on the trading system) in rather indirect terms and manner convey the inference gathered, that it is, as we may suppose, not a profitable speculation to them, or, more figuratively, the sweets of the hive are not exactly found in this track; and since the legislature has anticipated their privileged means in this respect, what comparatively they can do, but poorly compensates for the incidents attendant; for it is to be remarked, that it is not among a few independent but general mass of lives combined, that the calculations of contingencies are founded and applied; and as an apparent consequence, the latter condition may be expected to partake in a higher degree favourable in the determination of the happening and failing of events generally, as well as in reference to the claims that may in probability of expectation successively terminate.

These reflections naturally lead me then to inquire into the motive, why most of the chartered companies display and hold forth their legislative powers enrolled and enabling them to speculate in the grant and purchase of annuities, on just and equitable principles, when at the same time they evince a coldness,

Let us examine how Mr. Ricardo has come to the knowledge that "one foot of oil gas is equal to four of coal gas." This is the grand point of attraction held out to the admiration of the public; and one on which as flatly contradictory evidence has been borne, as distinguished the late memorable proceedings in the Court of Common Pleas. Thus, whilst Mr. Deville, of the Strand, is inclined to compare oil gas with coal gas in the ratio of 9 to 5, and while Mr. Brande, in his Manual of Chemistry, p. 156, says that "from two to three cubic feet may be regarded as equivalent to five or six of coal gas," we have every other intermediate proportion, even as high as Mr. Ricardo's *four to one*. Mr. R. says, "I have found that an Argand burner giving a light equal to six candles, six to the pound, consumed one cubical foot in the hour." What was the specific gravity of the gas made use of, or what the kind of candle (wax or tallow), the number of threads in each wick, or the weight of each consumed, we are not informed. This we know, that Professor Brande states that "an Argand burner of oil gas giving the light of eight wax candles (four to the pound) was found to consume 3900 cubical inches, or rather more than $2\frac{1}{4}$ cubic feet per hour."

But to proceed with Mr. R.'s statement, which is now brought into comparison with some results of Mr. Accum, who states, in his work on Gas Lights, p. 276, that "an Argand burner of coal gas giving a light equal to three candles (eight to the pound) consumes two cubical feet per hour." These are the species of data from which conclusions so satisfactory to Mr. Ricardo have been deduced! 'Tis well he refers us to p. 276, for at other pages of this genuine book-maker, I can find divers ratios to suit any theory! So much for the comparison of one discrepancy with another. But if *one* volume of oil gas is not equal to *four* of coal gas, of which Mr. R. himself seems to have some misgivings, for "three and a half" is ultimately the foundation upon which he raises his allegations against coal gas, surely it can be well authenticated as being equal to *three* volumes! for he says "the quantity of light produced from a given quantity of oil gas is stated by an eminent chemist to be equal to three times the quantity produced from coal gas."

Who this eminent chemist is, we are left to conjecture: it surely cannot be Dr. Henry *, who has written most elaborately on the subject, and more to the point than any one. Possibly it may be Professor Brande, because he said at the Bakerian lec-

* See papers of his in Nicholson's Journal for 1805, vol. xi; in the Transactions of the Royal Society for 1808; and in the last volume of the Manchester Memoirs. Another paper of his on the same interesting subject has lately been read by him before the Royal Society, which perhaps will shortly appear.

ture,

is no question of specific relation to the mathematical value; but let them understand, that every person so conversant with this subject is enabled to say, and knows as well as they, what compensation should at any time be returned; and what as parties to a contract, at least construed in equity, they are bound by ties of ordinary faith to give the assured member. Let us now contrast the preceding sums, and equal but to 93*l.* as what would be allowed in a propriety concern: let us, I say, compare the extraordinary difference in the value of an assurance equal in amount effected with the Equitable under almost precisely similar circumstances; viz. on the same life for 2000*l.* in 1[illegible]9, at an annual payment of 52*l.* 4*s.*; but it is right to add, there is on this a bonus of 600*l.* were it now a claim; as also of 150*l.* on policy No. 1, as I omitted to state. For the policy existing at the Equitable, then, the trustees will refund the sum of 514*l.* 9*s.* if surrendered any time before the annual premium due should be paid: so, allowing for this, the sum nominally offered is 566*l.* 13*s.* It is of no utility here to urge any thing as to the variation of the premiums, they being in the one case upon a single assurance, in the other apportioned upon three separately; it is the aggregate of the annual premiums and relative amount of assurance we are directed to, and that strikes as dissonant to reason; it is an instance, in which the mind discriminates truth and error from derived intuitive evidence, and not enveloped in the mists of sophistry.

Further concerning these two values: as referred to the first announced, the discordant medium or discrepancy between them is not easily, if at all, explicable by any order or process of computation on which such kind of deductions should ever be regulated and assumed as the only infallible in result. We may next, as not without its use, since elucidation is the mean intended, proceed to a different view of the subject; for, by placing the same proposition in new and opposite directions, we see clearer what is either apposite or inconsistent. It appears then on the three former-described policies, the gross amount of premiums paid is 663*l.* 11*s.* 8*d.*, or equal to 15, on the first; 13, on the second; 12, on the last; and these, if calculated as distinct annuities at 5 per cent., the first payment of each being made when the policy was opened, and so on at the beginning of every year succeeding, in which the life, it is considered, will be a claim—the equivalent amount is 952*l.* 3*s.* 6*d.* that is, equal in value now to an annuity of 47*l.* 12*s.* 2*d.* certain, to continue for ever, or in perpetuity at the same rate of interest, and in consideration of having received which, it is that the office proffers in signifi-

cant return the sum of barely 93*l*. Is this then a precedent distinguished by any principle of liberality? is it distinguished by a feature of justice? It would, I imagine, be an excess of candour to confess as much; but nay, more than this unjustly it seems, that at some offices there is a refusal to cancel at all, or rather to make any return whatever of the premiums received; and thus, were it not through the instrumentality of those trafficking in such things, it would be impossible to recover a single shilling of the immense sums so expended, and which, under the peculiar circumstance, would inevitably be sacrificed without any collateral advantage acquired. An observation occurs now, and though implied, yet should be expressly so, to the credit of the Society animadverted upon, it does then at recurring intervals divide with its members a part of the enormous gains of profit made; they do this in reality, and as what is falsely insinuated in many, where the scale of premiums is actually the same as at the Equitable, and yet never add a mite beyond the sum assured; and with this fact so glaring, it bewilders one to think how they should be able to effect any policies with the public at all, excepting by the medium of a corrupt influence, or a courtesy tantamount to servility which their most interested and strenuous supporters may command in the world. If in general this is the arbitrary and unfair practice dictated in all the subscription Societies, how are we to applaud or even give credence to the reiterated professions made with unblushing effrontery, that in all negotiations with the public, both its "interest and convenience are considered," being in all their proceedings actuated by a strict sense of rectitude and of honour?

I have in the present paper intentionally abstained from mixing up any extraneous calculations with it: by resolving algebraic into numerical values, the enunciation is simply apparent and intelligible to the general reader. I must now, in conformity with my original design, revert thereto, and introduce the table itself and auxiliary means of construction suited accordingly.

Table of the Value of an Annuity of 1l. increasing in Order of the Series 1. 2. 3. 4. .. n. on a Life at every Age from Three Years to extreme Duration. Computed from Observations of M. De Parcieux.

Age.	3 per Cent.	3½ per Cent.	4 per Cent.
3	458,393	389,494	333,422
4	463,367	394,275	337,923
5	464,747	396,027	339,846
6	464,227	396,174	340,404
7	462,201	395,042	339,870
8	459,116	393,018	338,564
9	455,451	390,488	336,841
10	450,679	387,006	334,288
11	444,794	382,562	330,899
12	437,806	377,158	326,685
13	430,743	371,679	322,397
14	423,606	366,125	318,02
15	416,399	360,497	313,585
16	409,123	354,797	[illegible]
17	402,261	349,445	304,853
18	395,327	344,016	300,57
19	388,319	338,511	296,244
20	381,24	332,932	291,813
21	374,555	327,683	287,657
22	367,792	322,354	283,423
23	360,951	316,945	279,108
24	354,031	311,454	274,712
25	347,036	305,881	270,23
26	339,962	300,227	265,662
27	332,813	294,49	261,016
28	325,589	288,672	256,286
29	318,292	282,765	251,471
30	310,922	276,782	246,571
31	303,48	270,718	241,585
32	295,969	264,575	236,513
33	288,389	258,352	231,355
34	280,744	252,052	226,111
35	273,035	245,674	220,782
36	265,266	239,222	215,369
37	257,439	232,695	209,872
38	249,186	225,762	203,988
39	242,07	218,775	198,035
40	233,807	211,7403	192,018
41	225,525	204,663	185,942
42	217,235	197,551	179,813
43	208,944	190,412	173,636
44	200,662	183,254	167,42
45	192,399	176,086	161,171
46	184,591	168,019	154,900

Table con-

TABLE continued.

Age.	3 per Cent.	3½ per Cent.	4 per Cent.
47	176,71	162,03	148,81
48	168,863	155,147	142,75
49	161,338	148,532	136,918
50	154,173	141,93	131,076
51	147,012	135,587	125,452
52	140,164	129,493	120,037
53	133,343	123,413	114,616
54	126,573	117,355	109,195
55	120,092	111,542	103,981
56	113,407	105,757	98,774
57	107,037	100,013	93,583
58	100,951	94,51	88,6
59	94,938	89,054	84,641
60	89,01	83,655	78,716
61	83,178	78,324	73,836
62	77,455	73,075	69,013
63	72,028	68,083	64,415
64	66,723	63,187	59,891
65	61,557	58,402	55,456
66	56,697	53,888	51,26
67	52,135	49,642	47,304
68	47,87	45,663	43,588
69	43,9	41,951	40,113
70	40,223	38,505	36,879
71	36,709	35,205	33,775
72	33,497	32,18	30,924
73	30,462	29,316	28,218
74	27,613	26,622	25,666
75	24,96	24,105	23,277
76	22,387	21,662	20,952
77	20,033	19,421	18,814
78	17,907	17,393	16,875
79	15,902	15,475	15,039
80	14,148	13,798	13,431
81	12,537	12,258	11,95
82	11,124	10,859	10,603
83	9,686	9,47	9,263
84	8,249	8,078	7,914
85	6,953	6,821	6,692
86	5,807	5,705	5,607
87	4,814	4,738	4,664
88	3,79	3,735	3,684
89	2,893	2,856	2,821
90	2,126	2,103	2,08
91	1,486	1,472	1,459
92	,9567	,9499	,943
93	,4854	,4881	,481
94	zero.	zero.	zero.

Respecting these numbers and tabulated values, which I have extended to other rates, but cannot well augment here, it is to be observed when at any time needed they are directed to be calculated by M. De Moivre's Hypothesis, from which some neat and appropriate formula has been investigated; but it is obvious the values deduced by it are only approximate to the true ones as found from the series of decrements of life; but this latter value however can be obtained, and, excluding the hypothesis, with greater ease, facility and accuracy of course, after the manner ensuing: first, having from real observations and for any order of increase determined the value of any such annuity upon an advanced life, or beginning from an age near the end of the table, then having for any form chosen so determined the value at the age begun with, we may thence assign in succession the value for every age intercepted, or that preceding, by aid of a subsidiary formula; and the operation being continued until the Table is completed, as also similarly for single, joint or longest of lives, the application may be generalized.

Enumerating therefore the formula of derivation. Of the order of numeral series in the table 1. 2. 3. 4. &c.

Let A signify the value of 1*l.* annuity on life N, at age $n-1$. B, that at age n, but increasing by 1. 2., 3*l.* Then the value of such annuity for life N, is expressed by $\left(A + B \times \frac{a'}{ax}\right)$.

If the annuity increases in the order 1. 3. 5. 7; if C be its value at age n, then is whole value for N $= 2A + (C-1).\frac{a'}{ax} = \left(A + \frac{2Ba'}{ax}\right)$.

If increasing by the triangular series 1. 3. 6. 10; and D be its value at age n, the value for N is $= A + \frac{(B+D)a'}{ax}$.

If it increases as the quadrangular series 1. 4. 9. 16, the value for N, $= A + \frac{(B+2D)a'}{ax}$.

Where in each theorem $\frac{a'}{ax}$ is the expectancy of 1*l.* on life N, a year hence. And, expanding by the combinatorial methods, other forms of series will be developed, and the formula of continuation applied. There are beside certain modifications fitting in practice, and some relative to the deduction of value in the case when an annuity is not for the whole term of duration, but temporary, or in remainder deferred; but these are not here admitting a full description.

This being the first table of the value of a life annuity increasing,

creasing, founded on any registered observations of mortality, that have as yet been published, the computist may lean with due confidence on its accuracy; it contains, I believe, no error of moment, the numbers respectively were derived from a double operation pursued, logarithmically and by calculation, the former to check and verify the latter; and though this is indispensable, yet the process by logarithms is not invariably critically exact in any specific inquiry, as will happen in fact from such tables, having been mostly by interpolation of differences compiled and extended: when then any result occurs so, as dissonant, it is only to be settled by a distinct revision, the work collated anew, or by some independent mode of verification.

Of all the chartered companies established, they avowedly profess to blend with their customary the life annuity business in every species of variety and form: it is unaccountably strange, then, they evince not partially a wish to promote the cause of that science, by which it is, that the stability of their plans is to be determined, and even existence vitally depend; and if we look unto those mushroom societies spontaneously emerging out of obscurity, ignorance or corrupt motives, it is but too visibly beheld that the age of bubble scheme and system has revived again, or is rapidly reviving: it would be a truly laudable task for any one possessing abstractedly the means, to devote some portion of time and talent to the discussion of their principles, views and measures; if however, and I must so qualify the expression, they can sustain this test and scrutiny of examination. I am aware of what is to be done, but, much as inclined, the engagement is such that few cheerfully and with alacrity would undertake and prosecute it through all its minutiæ of condition.

Aske Terrace, Hoxton, Feb. 1821. J. B. Benwell.

XXIV. *On the Solar and Lunar Periods.* *By Mr.* James Utting, *of Lynn Regis.*

To Mr. Tilloch.

Sir, — I have sent you for insertion in the Philosophical Magazine (if approved) a few extracts and remarks relative to the solar and lunar periods, as stated by modern astronomers, in order (in part) to meet the wishes of Mr. Yeates: as probably some of your astronomical correspondents are in possession of the means of a more extensive reference, my selections are but few, but I believe them to be the most correct published, agreeable to the present improved state of the science.

I remain, sir, yours truly,

Norfolk-street, Lynn Regis, Feb. 8, 1821. James Utting.

M. de

M. de la Lande. Astronomy, in 3 vols. Quarto, 3d Edit. Paris, 1792.

	Days.	hrs.	min.	sec.
Tropical year	365	5	48	48·
Sidereal do.	365	6	9	11·6
Tropical rev. of the ☽	27	7	43	4·6795
Sidereal do.	27	7	43	11·5259
Synodic do.	29	12	44	2·8283
Anomalistic do.	27	13	18	33·9499
Rev. of the ☽ to her ☊ ..	27	5	5	35·6030
Tropical rev. of the ☽'s perigee	3231	8	34	57·6177
Sidereal do.	3232	11	11	39·4089
Tropical rev. of the ☽'s ☊ ..	6798	4	52	52·0296
Sidereal do.	6793	7	13	17·7440

	°	′	″
Diurnal mot. of the ☽ to the equinox	13	10	35·027843940
Do. of the ☽'s perigee	0	6	41·069815195
Do. of the ☽'s ☊	0	3	10·638603696

The motion of the ☽ to the fixed stars being represented by unity, that of her perigee is .. .. } 0·008452264448

And that of her node .. 0·004021853526

	°	′	″
Sec. mot. of equinoctial points	1	23	45

M. Laplace. System of the World. Edit. 4th. Paris (Quarto, 1813).

	Days.	hrs.	min.	sec.
Tropical year	365	5	48	51·6096
Sidereal do.	365	6	9	11·5344
Sidereal rev. of the ☽	27	7	43	11·50107
Do. ☽'s perigee	3232	13	48	53·0496
Do. ☽'s ☊	6793	9	22	45·9840
Synodic rev. of the ☽	29	12	44	2·79914

	°	′	″
Sec. motion of equinoctial points ..	1	23	30

M. Delambre. Astronomy (Theoretical and Practical), 3 vols. Quarto, 1814.

	Days	hrs.	min.	sec.
Tropical year	365	5	48	50·
Sidereal do.	365	6	9	11·5344
Tropical rev. ☽	27	7	43	4·7183
Sidereal do.	27	7	43	11·8459
Anomalistic do.	27	13	18	35·
Synodic do.	29	12	44	2·8498
Rev. of the ☽ to her ☊ ..	27	5	5	36·
Rev. of the ☽'s perigee ..	3231	8	34	57·
Sec. motion of equinoctial points	1°	23′	30″	

M. Biot. Astronomy (Physical and Elementary), 2d Edit. 3 vols. Oct. 1811.

	Days.	hrs.	min.	sec.
Tropical year	365	5	48	51·6
Sidereal do.	365	6	9	11·5776
Sidereal rev.	27	7	43	11·5104
Anomalistic do.	27	13	18	37·44
Synodic do.	29	12	44	2·8032
Tropical rev. ☽'s perigee ..	3231	11	24	8·64
Sidereal do.	3232	13	56	12·48

Sec. mot. of equinoctial points .. 1° 23′ 30″

Burckhardt. Lunar Tables, Quarto. Paris, 1812.

	S.	°	′	″
Sec. mot. in long. in 100* Julian years	10	7	52	53·5
Do. of the ☽'s anomaly ..	6	18	49	5·3
Do. of the ☽'s ☊	4	14	10	12·0

Mr. Vince in the 2d edit. of his Complete System of Astronomy, 3 vols. quarto. 1814, has adopted the solar and lunar motions as given by M. de la Lande, in the 3d edition of his Astronomy, as before stated, which have also been copied into several of our modern astronomical works and Encyclopædias. In Taylor's Sexagesimal Tables, and in Hutton's Math. and Phil. Dictionary, the tropical year is stated at 365 days 5 hours 48 min. 45 sec. Bonnycastle's Astronomy states it at half a second more. Barlow (Phil. Dictionary); Edinburgh and Imperial Encyclopædias; Lalande (*Astronomie*); Laplace (*Systeme du Monde,* 1st edit.); Squire (Grammar of Astronomy); Vince (Astronomy); Woodhouse (Astro.); Whiting (Astronomy now publishing); and Dr. Young in his Treatise on Philosophy, in 2 vols. quarto, state the mean length of the solar year at 365 days 5 hours 48 min. 48 sec. Dr. O. G. Gregory (Astro.) states it at one second more. Mr. Burckhardt, 365 days 5 hours 48 min. 49·732 seconds. Laplace (*Systeme du Monde,* 4th edit.); Delambre (Solar Tables); Biot (Astronomy) state the solar year

* From which I have deduced the following periods:

	Days.	hrs.	min.	sec.
Trop. rev. of the ☽	27	7	43	4·70539
Sid. do.	27	7	43	11·53824
Anomalistic do.	27	13	18	33·35681
Synodic do.	29	12	44	2·84517
Rev. of the ☽ to her ☊ ..	27	5	5	35·71380
Trop. rev. of the ☽'s perigee ..	3231	10	56	57·02423
Sidereal do.	3232	13	30	33·93601
Trop. rev. of the ☽'s ☊ ..	6798	6	21	26·4000
Sidereal do.	6793	8	55	46·78841

Sec. mot. equinoctial points being taken at 1° 23′ 35″

at

at 365 days 5 hours 48 min. 51·6 seconds. M. Delambre in his Astronomy has finally fixed it at 365 days 5 hours 48 min. 50 sec. being nearly the same result as that obtained by M. Burckhardt. The tropical year of 365 days 5 hours 48 min. 48 sec. appears from the latter statements to be rather too little; but as the solar year is decreasing, it is evidently the best to be adopted in the reformation of the calendar—*Vide* my remarks on the proposal for establishing a more correct account of civil time, in Phil. Mag. vol. 55. p. 350. The true *solar year* at the commencement of the present century appears to be

	Days.	hrs.	min.	sec.
	365	5	48	50
Sid. year (*Syst. du Monde*, ed. 4th)	365	6	9	11·5344*.
Synodic period ☽ (do.) ..	29	12	44	2·8 (very nearly).

From which I have calculated the following particulars (and which I trust are as correct as any at present published).

	Circles.	S.	°	′	″
Sec. mot. of the ☉ to the equinox	100	0	0	45	51·5985
Do. of the ☉'s perigee .. .	0	0	1	43	18·0900
Do. of the ☉'s anomaly .. .	99	11	29	2	33·5085
Do. of the equinoctial points .	0	0	1	23	86·5872
Sid. and sec. motion ☉ .. .	99	11	29	22	15·0112
Do. .. .. ☽ ..	1336	10	6	29	44·7884
Sec. mot. of the ☽ to the equinox	1336	10	7	53	21·3756
Sec. mot. of the ☽ *a'* ☉ ..	1236	10	7	7	29·7771
Do. .. ☽'s perigee	11	3	19	2	51·9599
Do. .. ☽'s anomaly	1325	6	18	50	29·4157
Do. .. ☽'s ☊ ..		4	14	9	51·1904
Do. .. ☽ to her ☊	1342	2	22	3	12·5660
Do. .. ☉ to the ☽'s ☊	105	4	14	55	42·7889

	Days.	hrs.	min.	sec.
Trop. r . of the ☽ ..	27	7	43	4·666725
Sid. rev. of the ☽ ..	27	7	43	11·501745
Anom. rev. of the ☽ ..	27	13	18	33·239828
Synodic rev. of the ☽ ..	29	12	44	2·800000
Rev. of the ☽ to her ☊ ..	27	5	5	35·703570
Rev. of the ☉ to the ☽'s ☊	346	14	52	36·760457
Trop. rev. of the ☽'s perigee	3231	11	14	45·133018
Sid. rev. of the ☽'s perigee	3232	13	48	53 049600
Trop. rev. of the ☽'s ☊ ..	6798	6	50	41·814208
Sid. rev. of the ☽'s ☊ ..	6793	9	22	45·984000
One synodic rev. ☽ contains	29	12	44	2·8

	Days.	hrs.	min.	sec.
* N. B. The anomalistic year contains	365	6	13	59 24260

	Circle.	S.	°	′	″
Motion of the ☽'s anom. in this period	1	0	25	49	0·8491040724
Do. ☉'s anom. ..	0	0	29	6	19·2587471028
Do. ☉ to the ☽'s ☊	0	1	0	40	13·8721465646

Hence a Chaldean period of 223 lunations

	Days.	hrs.	min.	sec.
contains	6585	7	42	24·4
Nineteen rev. of the ☉ a′ ☽'s ☊ =	6585	18	39	58·4

	Circles.	S.	°	′	″
Motion of the ☽'s anom.	238	11	27	10	9·3502
Do. ☉'s anom.	18	0	10	29	34·7006
Dist. of the ☉ a′ ☽'s ☊	18	11	29	31	33·4887

Whence the comp. of the motion of the ☉ to the ☽'s ☊ in each Chaldean period amounts to 28′ 26″·5113, as I before stated.

XXV. *On the Division of the Circle into seventeen equal Parts.*

To Mr. Tilloch.

Sir, — A curious discovery lately made in pure mathematics, we owe to M. Gauss of Göttingen, who has shown, contrary to the opinion that has prevailed from the most ancient times, that a regular polygon of seventeen sides may be inscribed in a circle, without having recourse to any other principles than those admitted in the plane geometry. As the author's own solution of this problem is a part of a peculiar and very abstruse and recondite theory, the following communication, in which the problem is solved without any reference to that theory, may not be unacceptable.

Let $a = \frac{360°}{17} = 21° \frac{3}{17}$. If we set about solving the problem in the usual way, by seeking a value of cos. a, we shall obtain an equation of sixteen dimensions; because, in this mode of proceeding, the cosines of the several arcs, a, $2a$, $3a$, &c., sixteen in number, are not distinguished from one another, and are all comprehended indiscriminately in the same result. The equation thus obtained, although of sixteen dimensions, is however only of the eighth order, every two arcs that together make the whole circumference having the same cosine, viz. $\cos a = \cos 16a$, $\cos 2a = 15a$, $\cos 3a = \cos 14a$, &c.

Supposing n to denote any whole number, let $\phi = 4na$; then,

$$4\phi + \frac{\phi}{4} = n \times 17a = n \times 360°. \qquad (1)$$

where-

wherefore we have this equation, viz.

$$\text{Cos } 4\phi = \cos \frac{\phi}{4}. \quad (2)$$

Now,
$$\text{Cos } 2\phi = 2 \cos^2\phi - 1$$
$$\text{Cos } 4\phi = 2\,(2 \cos^2 \phi - 1)^2 - 1$$
$$\text{Cos } \phi = 2\,(2 \cos \frac{\phi}{4} - 1)^2 - 1:$$

wherefore, if we put $x = \cos 4\phi = \cos \frac{\phi}{4}$, and $y = \cos \phi$, we get these equations, viz.

$$\left.\begin{array}{l} x = 8y^4 - 8y^2 + 1 \\ y = 8x^4 - 8x^2 + 1 \end{array}\right\} \quad (3)$$

In these equations it is easy to prove that $x = \cos \phi = \cos 4na$, can never be equal to $y = \cos \frac{\phi}{4} = \cos na$: and since the cosines of all the multiples of a are only eight in number, it follows that there can be no more than four different values of x, and four corresponding values of y, that satisfy the equations. In order to illustrate this, let all the values of ϕ and $\frac{\phi}{4}$, that have different cosines, be written in two lines, viz.

$$\begin{array}{lcccccccc} \phi: & 4a, & 8a, & 12a, & 16a, & 20a, & 24a, & 28a, & 32a, \\ & & & & 5a & a & 3a & 7a & 6a \quad 2a, \\ \frac{\phi}{4}: & a, & 2a, & 3a, & 4a, & 5a, & 6a, & 7a, & 8a, \end{array}$$

the arcs placed below in the first line have the same cosines with those above, being found by taking the difference between $17a$, or $34a$. Now, if we compare the cosines of the corresponding arcs in the two lines, it will readily appear that we shall have all the different values of x and y, by making ϕ equal to one or other of the four arcs,

$$4a,\ 8a,\ 12a,\ 24a:$$

for when we make ϕ equal to

$$16a,\ 20a,\ 28a,\ 32a,$$

we obtain the same values of x and y as before.

What has been said relates to the values of ϕ in multiples of the arc a; values which are exclusively determined by means of the two equations,

$$\text{Cos } 4\phi = \cos \frac{\phi}{4}$$

$$\text{Sin } 4\phi = - \sin \frac{\phi}{4},$$

both deduced from equation (1). But if $b = \frac{360^\circ}{15} = 24^\circ$, and $\phi = 4nb$, then

$$4\phi - \frac{\phi}{4} = n \times 360^\circ, \qquad \text{whence}$$

whence we get

$$\text{Cos } 4\phi = \cos \frac{\phi}{4}$$

$$\text{Sin } 4\phi = \sin \frac{\phi}{4}.$$

In order to find how many different solutions of the equations (3) are derived from the multiples of b, let all the values of ϕ and $\frac{\phi}{4}$ that have different cosines be written in two lines, viz.

$$\begin{array}{cccccccc} \phi: & 4b, & 8b, & 12b, & 16b, & 20b, & 24b, & 28b, \\ & 7b, & 3b, & b, & 5b, & 6b, & 2b, \\ \frac{\phi}{4}: & b, & 2b, & 3b, & 4b, & 5b, & 6b, & 7b. \end{array}$$

From this it is evident that in four instances, viz. when $\phi = 4b$, $\phi = 8b$, $\phi = 16b$, and $\phi = 28b$; $\cos \phi$ and $\cos \frac{\phi}{4}$, that is, x, and y, are unequal: but these four instances give no more than two values of x and two corresponding values of y in the equations (3). In the other three instances, viz. when $\phi = 12b$, $\phi = 20b$, and $\phi = 24b$; it appears that $\cos \phi = \cos \frac{\phi}{4}$, or $x = y$; and in these cases the two equations (3) coincide in one, viz.

$$x = 8x^4 - 8x^2 + 1,$$

that is $$8x^4 - 8x^2 - x + 1 = 0,$$

or, which is the same,

$$(4x^2 + 2x - 1)\ (2x + 1)\ (x - 1) = 0.$$

Now, in the equation $4x^2 + 2x - 1 = 0$, the values of x are $x = \cos 12b = \cos 3b$, and $x = \cos 24b = \cos 6b$; and these determine the points of the quindecagon that coincide with the pentagon. In the factor $2x + 1 = 0$, $x = \cos 20b = \cos 5b$; and this gives the points of the quindecagon coinciding with the equilateral triangle. The remaining factor $x - 1 = 0$, corresponds to the cases when ϕ and $\frac{\phi}{4}$ are either both zero, or both multiples of 360°.

On the whole, setting aside the particular case when $x = y$, the complete solution of the equations (3) will give six different values of x and as many corresponding values of y. Of these twelve values, two of x and two of y determine those eight points of the quindecagon that coincide neither with the pentagon nor the equilateral triangle; and the remaining values of x and y determine all the points of the polygon of seventeen sides.

In order to solve the equations (3), let $x = \frac{m+n}{4}$, $y = \frac{m-n}{4}$; then, by substitution,

$$m-n = \frac{(m+n)^4}{8} - 2\,(m+n)^2 + 4$$

$$m+n = \frac{(m-n)^4}{8} - 2(m-n)^4 + 4\,;$$

and, by adding and subtracting these,

$$8m = m^4 + 6m^2 n^2 + n^4 - 16m^2 - 16n^2 + 32$$
$$-2n = m^3 n + mn^3 - 8mn.$$

From the second of these equations we get,

$$n^2 = 8 - \frac{2}{m} - m^2,$$

$$n^4 = 64 + \frac{4}{m^2} - \frac{32}{m} + 4m - 16m^2 + m^4\,;$$

and these values being substituted in the first equation, we shall obtain after reducing,

$$m^6 - 8m^4 + 4m^3 + 8m^2 - 1 = 0.$$

Divide all the terms of this equation by m^3, then

$$m^3 - \frac{1}{m^3} - 8m + \frac{8}{m} + 4 = 0\,;$$

and if we now put $z = m - \frac{1}{m}$, and substitute, we shall obtain,

$$z^3 - 5z + 4 = 0,$$

or,

$$(z-1)\,(z^2 + z - 4) = 0.$$

From the factor $z - 1 = 0$, we get $z = 1$, and hence

$$m - \frac{1}{m} = 1$$

$$n^2 = 8 - \frac{2}{m} - m^2$$

$$x = \frac{m+n}{4},\ y = \frac{m-n}{4}.$$

These formulæ determine two values of each of the quantities m, n, x, y; and the values of x and y so obtained, are the cosines which, as has been shown above, belong to the quindecagon.

For the polygon of seventeen sides, we have

$$z^2 + z - 4 = 0$$

$$m - \frac{1}{m} = z$$

$$n^2 = 8 - \frac{2}{m} - m^2$$

$$x = \frac{m+n}{4},\ y = \frac{m-n}{4}.$$

Now, if we put $p = \frac{-1+\sqrt{17}}{4}$, $p' = \frac{-1-\sqrt{17}}{4}$; the two values

of

of z in the foregoing quadratic equation, will be $2p$ and $2p'$: and hence

$$m - \frac{1}{m} = 2p$$

$$m - \frac{1}{m} = 2p';$$

from which we derive four values of m, viz.

$$m = p + \sqrt{p^2 + 1}$$

$$m' = p - \sqrt{p^2 + 1} = -\frac{1}{m}$$

$$m'' = p' + \sqrt{p'^2 + 1}$$

$$m''' = p' - \sqrt{p'^2 + 1} = -\frac{1}{m''}:$$

And, by substituting these values in the formula for n^2, we get four corresponding values of n, viz.

$$n = \sqrt{8 + 2m' - m^2}$$

$$n' = \sqrt{8 + 2m - m'^2}$$

$$n'' = \sqrt{8 + 2m''' - m''^2}$$

$$n''' = \sqrt{8 + 2m'' - m'''^2}:$$

And finally, from the values of m and n now found, we deduce all the values of x and y, or the eight cosines that determine the points of a polygon of seventeen sides inscribed in a circle, viz.

$$\frac{m+n}{4}, \qquad \frac{m-n}{4},$$

$$\frac{m'+n'}{4}, \qquad \frac{m'-n'}{4},$$

$$\frac{m''+n''}{4}, \qquad \frac{m''-n''}{4}$$

$$\frac{m'''+n'''}{4}, \qquad \frac{m'''-n'}{4}.$$

The numerical values of all the cosines sought are thus found; but the investigation does not determine to which arc any numerical value belongs. This must be made out by means of the relative magnitude of the quantities; the largest number answering to the greatest cosine. One thing only we learn from the analysis, which is, that every two corresponding numbers, or two found by adding and subtracting the same values of m and n, belong to two arcs, one of which is quadruple of the other.

A. B.

XXVI. *Some*

XXVI. *Some Account of a Method which may be applied to the same Purposes as Sir* Isaac Newton's *Method of Fluxions. By Mr.* Thomas Tredgold.

Letter I.

To Mr. Tilloch.

Sir, — You are well aware that there are not many mathematicians who are perfectly satisfied with the manner of establishing the first principles of the fluxional calculus; and the logical basis of the differential calculus is still more objectionable. Perhaps the method, which I intend partly to explain in this letter, will be thought less objectionable in its principles, and that these principles being more obvious it may be applied with greater certainty, and be more capable of improvement. I submit it however, with diffidence, to the opinions of those who have more extended knowledge, and a greater share of experience.

My method will be best explained by showing examples of its application, which with your permission I will lay before your readers, successively at such times as may be most convenient to myself; applying it in the first instance to the quadrature of curves.

Let x be the abscissa, and y the corresponding ordinate of a curve, and let the relation of x to y be expressed by the equation $xy = ax^n$. Also, suppose the abscissa to be divided into m equal parts, and an ordinate to be drawn at each point of division. It is obvious that the distance between the points of division will be equal to $\frac{x}{m}$, and the abscissas, and their corresponding ordinates, may be expressed as under, for each point of division.

$$\text{Abscissas}\quad \frac{x}{m},\quad \frac{2x}{m},\quad \frac{3x}{m},\quad \frac{4x}{m},\quad \ldots\ \ldots\quad \frac{mx}{m}.$$

$$\text{Ordinates}\quad \frac{ax^n}{m^n},\quad a\left(\frac{2x}{m}\right)^n,\quad a\left(\frac{3x}{m}\right)^n,\quad a\left(\frac{4x}{m}\right)^n,\quad \ldots\quad a\left(\frac{mx}{m}\right)^n.$$

Now, if the arithmetical mean between two adjoining ordinates be multiplied by the distance between them, and this distance be very small, the product will not sensibly differ from the true area of the curvilinear space, of which the two sides are bounded by the two ordinates. Therefore, the area of the curvilinear space, which is bounded by the ordinate y, abscissa x, and the curve, may be expressed by the series;

$$\frac{(0^n + ax^n)x}{2m^{n+1}} + \frac{(x^n + 2^n x^n)ax}{2m^{n+1}} + \frac{(2^n x^n + 3^n x^n)ax}{2m^{n+1}} + \ldots \frac{(\overline{m-1}^n x^n + m^n x^n)ax}{2m^{n+1}}.$$

$$\text{Or, } ax^{n+1} \times \frac{1^n + 2^n + 3^n + 4^n + \ldots\ \ldots\ \ldots\ (m-1)^n + \frac{1}{2}m^n}{m^{n+1}}.$$

But it will be shown in my next letter that m may be taken of such a magnitude that the sum of the series $1^n + 2^n + 3^n + 4^n + \ldots \ldots \frac{1}{2}m^n$ will not sensibly differ from $\frac{2m^{n+1}}{n+1}$; and at the same time the distance $\frac{x}{m}$ will be so inconceivably small that $\frac{ax^{n+1}}{n+1}$ will express the true area of the curve.

If the ordinate y be expressed by $ax^n \pm b$, then $\frac{ax^{n+1}}{n+1} \pm bx =$ the area.

It would be easy to multiply examples, which would afford a happy illustration of many of the methods of obtaining fluents; as it is easy to perceive that the $\dot{x}$ of the method of fluxions is equivalent to $\frac{x}{m}$ in this method. It will also be observed that if the series does not begin from zero a correction will be necessary, and may be made in the same manner as in fluxions.

In the preceding illustration the ordinates are supposed to be perpendicular to the abscissas; should they be inclined at any angle c, the area would be expressed by $\frac{\sin. c \times ax^{n+1}}{n+1}$.

What has been shown respecting the quadrature of curves is equally applicable to other objects of calculation, such as the contents of solids, mechanical effects, and inquiries of a like nature. In this letter I shall only offer another example, which is to find the length of a curve.

Let the abscissa be divided into m parts, and to each point of division let an ordinate be drawn perpendicular to the abscissa. Then, if the square of the difference between two ordinates, at the distance $\frac{x}{m}$ apart, be added to the square of $\frac{x}{m}$ the sum will be equal to the square of the small portion of the curve intercepted between those ordinates.

Hence, if $y = x^n$, the length z of the curve may be exhibited as follows:

$$z = \left(\frac{x^2}{m^2} + \frac{x^{2n}}{m^{2n}}\right)^{\frac{1}{2}} + \left(\frac{x^2}{m^2} + \frac{\overline{2^n x^n - x^n}^2}{m^{2n}}\right)^{\frac{1}{2}} + \ldots \left(\frac{x^2}{m^2} + \frac{\overline{m^n x^n - \overline{m-1}^n x^n}^2}{m^{2n}}\right)^{\frac{1}{2}}.$$

$$\text{Or } z = \frac{x}{m} \times \left[\left(1 + \frac{x^{2n-2}}{m^{2n-2}}\right)^{\frac{1}{2}} + \left(1 + \frac{\overline{2^n - 1}^2 x^{2n-2}}{m^{2n-2}}\right)^{\frac{1}{2}} + \&c.\right].$$

The progression will generally become more complicated when the

the equation of the curve is so; it is not however my object to show how such progressions may be summed; but to direct the attention of those who have more leisure to the subject.

There yet remain several illustrations of the application of similar principles to problems which are usually done by the method of fluxions; which will form the subjects of future communications. I am, sir, yours, &c.

No. 2, Grove Terrace, Lisson Grove, New Road, Feb. 16, 1821. THOMAS TREDGOLD.

[For a Continuation see p. 200.]

XXVII. *Lunar Tables: being an Appendix to pages* 244, 344, 439, *of Vol.* II.—*p.* 17, 81, 278, 354, *of Phil. Magazine* 1820.

TABLE I. The mean Motion of the Moon for 48 Cycles, containing a Period of 912 Years.

Years and Cycles.		Reduction of Julian Time.		Mean Motion of the Node.			Mean Motion of the Apogee.			Revolution of the Node.		Lunar Years and Months.	
C.	Y.	D.	H.	S.	D.	M.	S.	D.	M.			Y.	M.
0	0	Mar. 21	0	12	0	0	0	0	0	Mar.	Sept.	0	0
1	19		10½	11	22	30	1	22	30			19	7
2	38		9	11	15	0	3	15	0			39	2
3	57		7½	11	7	30	5	7	30			58	9
4	76		6	11	0	0	7	0	0	Feb.	Aug.	78	4
5	95		4½	10	22	30	8	22	30			97	11
6	114		3	10	15	0	10	15	0			117	6
7	133		1½	10	7	30	0	7	30			137	1
8	152	Mar. 20	0	10	0	0	2	0	0	Jan.	July.	156	8
9	171		10½	9	22	30	3	22	30			176	3
10	190		9	9	15	0	5	15	0			195	10
11	209		7½	9	7	30	7	7	30			215	5
12	228		6	9	0	0	9	0	0	Dec.	Jan.	235	0
13	247		4½	8	22	30	10	22	30			254	7
14	266		3	8	15	0	0	15	0			274	2
15	285		1½	8	7	30	2	7	30			293	9
16	304		0	8	0	0	4	0	0	Nov.	May.	313	4
17	323		10½	7	22	30	5	22	30			332	11
18	342		9	7	15	0	7	15	0			352	6
19	361		7½	7	7	30	9	7	30			372	1
20	380		6	7	0	0	11	0	0	Oct.	April.	391	8
21	399		4½	6	22	30	0	22	30			411	3
22	418		3	6	15	0	2	15	0			430	10
23	437		1½	6	7	30	4	7	30			450	5
24	456	Mar. 19	0	6	0	0	6	0	0	Sept.	Mar.	470	0
25	475		10½	5	22	30	7	22	30			489	7
26	494		9	5	15	0	9	15	0			509	2
27	513		7½	5	7	30	11	7	30			528	9
28	532		6	5	0	0	1	0	0	Aug.	Feb.	548	4
29	551		4½	4	22	30	2	22	30			567	11

 TABLE con-

TABLE continued.

Years and Cycles.		Reduction of Julian Time.		Mean Motion of the Node.			Mean Motion of the Apogee.			Revolution of the Node		Lunar Years and Months.	
C.	Y.	D.	H.	S.	D.	M.	S.	D.	M.			Y	M.
30	570		3	4	15	0	4	15	0			587	6
31	589		1½	4	7	30	6	7	30			607	1
32	608		0	4	0	0	8	0	0	July.	Jan.	626	8
33	627		10½	3	22	30	9	22	30			646	3
34	646		9	3	15	0	11	15	0			665	10
35	665		7½	3	7	30	1	7	30			685	5
36	684		6	3	0	0	3	0	0	June,	Dec.	705	0
37	703		4½	2	22	30	4	22	30			724	7
38	722		3	2	15	0	6	15	0			744	2
39	741		1½	2	7	30	8	7	30			763	9
40	760	Mar. 18	0	2	0	0	10	0	0	May,	Nov.	783	4
41	779		10½	1	22	30	11	22	30			802	11
42	798		9	1	15	0	1	15	0			822	6
43	817		7½	1	7	30	3	7	30			842	1
44	836		6	1	0	0	5	0	0	April,	Oct.	861	8
45	855		4½	0	22	30	6	22	30			881	3
46	874		3	0	15	0	8	15	0			900	10
47	893		1½	0	7	30	10	7	30			920	5
48	912		0	0	0	0	12	0	0	Mar.	Sept.	940	0

TABLE II. The Mean Motion of the Moon for one Cycle of xix Years.

Years.	Reduction of Julian Time.			Mean Motion of the Mooon.			Mean Motion of the Node.			Mean Motion of the Apogee.			Apogee Distance from Node.
	H.	M.	S.	S.	D.	M.	S.	D.	M.	S.	D.	M.	Signs.
0	12	0	0	0	0	0	0	0	0	0	0	0	0
1	11	55	16	4	12	38	0	19	20	1	10	40	2
2	11	50	32	8	25	16	1	8	41	2	21	19	4
3	11	45	48	1	7	54	1	28	1	4	1	59	6
4	11	41	4	5	20	32	2	17	22	5	12	38	8
5	11	36	20	10	3	10	3	6	42	6	23	18	10
6	11	31	36	2	15	48	3	26	3	8	3	57	0
7	11	26	52	6	28	26	4	15	23	9	14	37	2
8	11	22	8	11	11	4	5	4	44	10	25	16	4
9	11	17	24	3	23	42	5	24	4	0	5	56	6
10	11	12	40	8	6	20	6	13	25	1	16	35	8
11	11	7	56	0	18	58	7	2	45	2	27	15	10
12	11	3	12	5	1	36	7	22	6	4	7	54	0
13	10	58	58	9	14	14	8	11	26	5	18	34	2
14	10	53	44	1	26	52	9	0	47	6	29	13	4
15	10	49	0	6	9	30	9	20	7	8	9	53	6
16	10	44	16	10	22	8	10	9	28	9	20	32	8
17	10	39	32	3	4	46	10	28	48	11	1	12	10
18	10	34	48	7	17	24	11	18	10	0	11	51	0
19	10	30	0	12	0	0	0	7	30	1	22	30	2
20	10	25	16	4	12	40	0	26	50	3	3	10	4

TABLE III. The Mean Motion of the Sun and Moon for xix Lunations.

	Mean Lunations				Sun's Mean Anomaly.			Moon's Mean Anomaly.			Sun's Distance from Node.		
	Days.	H.	M.	S.	S.	D.	M.	S.	D.	M.	S.	D.	M.
1	29	12	44	2½	0	29	6	0	25	55	1	0	40
2	59	1	28	5	1	28	12	1	21	50	2	1	20
3	88	14	12	7	2	27	18	2	17	45	3	2	0
4	118	2	56	10	3	26	25	3	13	40	4	2	40
5	147	15	40	12	4	25	31	4	9	26	5	3	21
6	177	4	24	15	5	24	37	5	5	31	6	4	1
7	206	17	8	17	6	23	44	6	1	25	7	4	40
8	236	5	52	20	7	22	50	6	27	21	8	5	21
9	265	18	36	22	8	21	57	7	23	17	9	6	2
10	295	7	20	25	9	21	43	8	19	2	10	6	42
11	324	20	4	27	10	20	9	9	15	7	11	7	22
12	354	8	48	30	11	19	15	10	11	2	0	8	2
13	383	20	32	32	0	18	22	11	6	58	1	8	43
14	413	9	16	33	1	17	28	0	2	53	2	9	23
15	442	22	0	35	2	16	34	0	28	48	3	10	3
16	472	10	44	38	3	15	41	1	24	43	4	10	43
17	501	23	28	40	4	14	47	2	20	28	5	11	23
18	531	12	12	43	5	13	53	3	16	34	6	12	4
19	560	0	56	45	6	13	0	4	12	29	7	12	44

TABLE IV. The Measure and Equation of the Years according to the Motions of the Sun and Moon for xix Years.

	I. Solar Years.			II. Solilunar Years.				III. Julian Do.		IV. [illegible]
	Days.	H.	M	Days.	H.	M.	S.	Days.	H.	[illegible]
1	365	5	49	365	5	55	16	365	6	[illegible]
4	1460	23	16	1460	23	41	4	1461	0	1461
19	6939	14	31	6939	16	30	4	6939	18	6939

	V. Lunar Years.						VI. Equated Do.			VII. Calendar Do.	
	Days.	H.	M.	″	‴	⁗	Days.	H	Epacts.	Days.	Epacts.
1	354	8	48	30	38	17	354	6	+ 11	354	+ 11
	354	&c.					354	6	+ 11	354	+ 11
	354	&c.					354	6	+ 11	354	+ 11
	354	&c.					354	6	+ 11	355	+ 11
4	1417	17	14	2	33	8	1417	0	+ 44	1417	+ 44
19	6732	23	21	42	7	42	6730	18	+ 209	6730	+ 209
	+206	17	8	17	52	18	+209			+209	+
	6939	16	30				6939	18		6939	

Table V. showing the Progression of Solar and Lunar Eclipses for xix Years*.

Years.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1			☉ ☽						☉ ☽	☉		
2			☉ ☽					☉	☽			
3		☉						☉				
4	☽	☉						☉				
5	☉ ☽ ☉					☉	☽ ☉					☉
6	☽					☉						☉
7					☽	☉					☽ ☉	
8					☽ ☉					☉	☽	
9				☉ ☽						☉ ☽		
10				☉					☉			
11			☉						☉			
12		☉ ☽	☉					☉ ☽	☉			
13	☉	☽					☉	☽				
14	☉						☉					☽
15	☉					☽	☉					☽ ☉
16					☉	☽					☉	☽
17					☉						☉	
18				☽	☉					☽ ☉		
19				☽ ☉					☉	☽		
20			☉ ☽						☉ ☽			

* Epoch of this Table 1801, 1820, 1839.

XXVIII. *A Table of the Sun's Right Ascension in Time to every Ten Minutes of his Longitude: with the Differences and Secular Variation for Jan.* 1, 1801. (*Obliq. of the Eclip.* 23° 27′ 57″, *and Sec. Var.* 52″·1.) *Calculated from* TAYLOR's *Tables of Logarithms. By Mr.* JAMES UTTING, *Lynn Regis.*

[Continued from p. 32.]

Argu-

Argument. ⊙'s Long.	Signs 0 and VI. R. A.	Diff.	Sec. Var.	Signs I. and VII. R. A.	Diff.	Sec. Var.	Signs II. and VIII. R. A.	Diff.	Sec. Var.	Argument. ⊙'s Long.
° ′	h ′ ″	″	″	h ′ ″	″	″	h ′ ″	″	″	° ′
0 0	0 0 0·00	36·69	0·00	1 51 37·39	38·21	0·62	3 51 15·25	41·65	0·68	30 0
10	0 0 36·69	36·69	0·00	1 52 15·60	38·23	0·63	3 51 56·90	41·68	0·68	50
20	0 1 13·38	36·70	0·01	1 52 53·83	38·25	0·63	3 52 38·58	41·69	0·68	40
30	0 1 50·08	36·69	0·01	1 53 [illegible]	38·26	0·63	3 53 20·27	41·71	0·67	30
40	0 2 26·77	36·69	0·02	1 54 [illegible]	38·28	0·63	3 54 1·98	41·73	0·67	20
50	0 3 3·46	36·70	0·02	1 54 [illegible]	38·30	0·63	3 54 43·71	41·75	0·67	10
1 0	0 3 40·16	36·69	0·02	1 55 [illegible]	38·31	0·64	3 55 25·46	41·76	0·67	29 0
10	0 4 16·85	36·69	0·03	1 56 5·23	38·32	0·64	3 56 7·22	41·79	0·66	50
20	0 4 53·54	36·70	0·03	1 56 43·55	38·35	0·64	3 56 49·01	41·80	0·66	40
30	0 5 30·24	36·70	0·04	1 57 [illegible]	38·36	0·64	3 57 30·81	41·82	0·66	30
40	0 6 6·94	36·69	0·04	1 58 [illegible]	38·38	0·65	3 58 12·63	41.84	0·66	20
50	0 6 43·63	36·70	0·04	1 58 38·64	38·39	0·65	3 58 54·47	41·86	0·66	10
2 0	0 7 20·33	36·70	0·05	1 59 17·03	38·41	0·65	3 59 36·33	41·88	0·65	28 0
10	0 7 57 03	36·70	0·05	1 59 55·44	38·43	0·65	4 0 18·21	41·89	0·65	50
20	0 8 33·73	36·70	0·06	2 0 33·87	38·44	0·65	4 1 0·10	41·91	0·65	40
30	0 9 10·43	36·71	0·06	2 1 12·31	38·46	0·66	4 1 42·01	41·94	0·65	30
40	0 9 47·14	36·70	0·06	2 1 50·77	38·48	0·66	4 2 23·95	41·94	0·65	20
50	0 10 23·84	36.71	0·07	2 2 29·25	38·49	0·66	4 3 5·89	41·97	0·64	10
3 0	0 11 0·55	36·71	0·07	2 3 7·74	38·51	0·66	4 3 47·86	41·99	0·64	27 0
10	0 11 37·26	36·71	0·08	2 3 46·25	38·52	0·66	4 4 29·85	42·00	0·64	50
20	0 12 13·97	36·71	0·08	2 4 24·77	38·56	0·67	4 5 11·85	42·02	0·64	40
30	0 12 50·68	36·72	0 09	2 5 3·33	38·56	0·67	4 5 53·87	42·04	0·63	30
40	0 13 27·40	36·71	0·09	2 5 41·89	38·58	0·67	4 6 35·91	42·05	0·63	20
50	0 14 4·11	36·72	0·09	2 6 20·47	38·60	0·67	4 7 17·96	42·08	0·63	10
4 0	0 14 40·83	36·72	0·10	2 6 59·07	38 61	0·67	4 8 0·04	42·09	0·63	26 0
10	0 15 17·55	36·73	0·10	2 7 37·68	38·64	0·68	4 8 42·13	42·11	0·62	50
20	0 15 54·28	36·72	0·10	2 8 16·32	38·64	0 68	4 9 24·24	42·12	0·62	40
30	0 16 31·00	36·73	0·11	2 8 54 96	38·67	0·68	4 10 6·36	42·15	0·62	30
40	0 17 7·73	36·74	0·11	2 9 33·63	38·69	0·68	4 [illegible] 48·51	42·16	0·61	20
50	0 17 41·47	36·73	0·12	2 10 12·32	38·70	0·68	4 [illegible] 30·67	42·18	0·61	10
5 0	0 18 21·20	36·74	0·12	2 10 51·02	38·72	0·69	4 12 12·85	42·19	0·61	25 0
10	0 18 57 94	36·74	0·12	2 11 29·74	38·74	0·69	4 12 55·04	42·21	0·60	50
20	0 19 34·68	36·74	0·13	2 12 8·48	38·75	0·69	4 13 37·25	42·23	0·60	40
30	0 20 11·42	36·75	0·13	2 12 47·23	38·78	0·69	4 14 19·48	42·25	0·60	30
40	0 20 48·17	36·75	0·13	2 13 26·01	38·79	0·69	4 15 1·73	42·26	0·60	20
50	0 21 24·92	36·75	0·14	2 14 4·80	38·81	0·69	4 15 43·99	42·28	0·59	10
6 0	0 22 1·67	36·76	0·14	2 14 43·61	38·83	0·70	4 16 26·27	42·31	0·59	24 0
10	0 22 38·43	36·76	0·15	2 15 22·44	38·84	0·70	4 17 8·58	42·30	0·59	50
20	0 23 15·19	36·77	0·15	2 16 1·28	38·87	0·70	4 17 50·88	42·33	0·58	40
30	0 23 51·96	36·77	0·15	2 16 40·15	38·88	0·70	4 18 33·21	42·34	0·58	30
40	0 24 28·73	36·77	0·16	2 17 19·03	38·90	0·70	4 19 15·55	42·37	0·58	20
50	0 25 5·50	36·77	0·16	2 17 57·93	38·92	0·70	4 19 57·92	42·38	0·58	10
7 0	0 25 42·27	36·79	0·17	2 18 36·85	38·93	0·70	4 20 40·30	42·39	0·57	23 0
10	0 26 19·06	36·78	0·17	2 19 15·78	38·96	0·71	4 [illegible]·69	42·42	0·57	50
20	0 26 55·84	36·79	0·18	2 19 54·74	38·97	0·71	4 22 5·11	42·42	0·57	40
30	0 27 32·63	36·79	0·18	2 20 33·71	38·99	0·71	4 22 47·53	42·44	0·57	30
40	0 28 9·42	36·80	0·18	2 21 12·70	39·01	0·71	4 23 29·97	42·46	0·56	20
50	0 28 46·22	36·80	0·19	2 21 51·71	39 03	0·71	4 24 12·43	42·48	0·56	10
8 0	0 29 23·02	36·81	0·19	2 22 30·74	39·05	0·71	4 24 54·91	42·49	0 56	22 0
	Signs V. and XI.			Signs IV and X.			Signs III. and IX.			

TABLE continued.

Argu-ment.	Signs 0 and VI.			Signs I. and VII.			Signs II. and VIII.			Argu-ment.
⊙'s Long.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	⊙'s Long.
° ′	h ′ ″	″	″	h ′ ″	″	″	h ′ ″	″	″	° ′
10	0 29 59·83	36·81	0·19	2 23 9·79	39·07	0·71	4 25 37·40	42·50	0·55	50
20	0 30 36·64	36·82	0·20	2 23 48·86	39·08	0·72	4 26 19·90	42·53	0·55	40
30	0 31 13·46	36·82	0·20	2 24 27·94	39·10	0·72	4 27 2·43	42·53	0·55	30
40	0 31 50·28	36·83	0·21	2 25 7·04	39·13	0·72	4 27 44·96	42·56	0·54	20
50	0 32 27·11	36·83	0·21	2 25 46·17	39·14	0·72	4 28 27·52	42·57	0·54	10
9 0	0 33 3·94	36·84	0·21	2 26 25·31	39·16	0·72	4 29 10·09	42·58	0·54	21 0
10	0 33 40·78	36·84	0·22	2 27 4·47	39·18	0·72	4 29 52·67	42·60	0·53	50
20	0 34 17·62	36·85	0·22	2 27 43·65	39·19	0·72	4 30 35·27	42·61	0·53	40
30	0 34 54·47	36·85	0·23	2 28 22·84	39·22	0·73	4 31 17·88	42·63	0·53	30
40	0 35 31·32	36·86	0·23	2 29 2·06	39·24	0·73	4 32 0·51	42·64	0·52	20
50	0 36 8·18	36·87	0·24	2 29 41·30	39·25	0·73	4 32 43·15	42·66	0·52	10
10 0	0 36 45·05	36·87	0·24	2 30 20·55	39·27	0·73	4 33 25·81	42·68	0·52	20 0
10	0 37 21·92	36·88	0·24	2 30 59·82	39·30	0·73	4 34 8·49	42·68	0·52	50
20	0 37 58·80	36·88	0·24	2 31 39·12	39·31	0·73	4 34 51·17	42·71	0·51	40
30	0 38 35·68	36·89	0·25	2 32 18·43	39·33	0·73	4 35 33·88	42·71	0·51	30
40	0 39 12·57	36·89	0·25	2 32 57·76	39·35	0·73	4 36 16·59	42·73	0·50	20
50	0 39 49·46	36·91	0·26	2 33 37·11	39·37	0·73	4 36 59·32	42·75	0·50	10
11 0	0 40 26·37	36·90	0·26	2 34 16·48	39·39	0·73	4 37 42·07	42·76	0·50	19 0
10	0 41 3·27	36·92	0·26	2 34 55·87	39·41	0·74	4 38 24·83	42·77	0·49	50
20	0 41 40·19	36·92	0·27	2 35 35·28	39·43	0·74	4 39 7·60	42·79	0·49	40
30	0 42 17·11	36·93	0·27	2 36 14·71	39·44	0·74	4 39 50·39	42·80	0·49	30
40	0 42 54·04	36·93	0·28	2 36 54·15	39·47	0·74	4 40 33·19	42·82	0·48	20
50	0 43 30·97	36·94	0·28	2 37 33·62	39·49	0·74	4 41 16·01	42·82	0·48	10
12 0	0 44 7·91	36·95	0·28	2 38 13·11	39·50	0·74	4 41 58·83	42·85	0·47	18 0
10	0 44 44·86	36·96	0·29	2 38 52·61	39·53	0·74	4 42 41·68	42·85	0·47	50
20	0 45 21·82	36·96	0·29	2 39 32·14	39·54	0·74	4 43 24·53	42·87	0·47	40
30	0 45 58·78	36·97	0·29	2 40 11·68	39·57	0·74	4 44 7·40	42·88	0·46	30
40	0 46 35·75	36·98	0·30	2 40 51·25	39·58	0·74	4 44 50·28	42·90	0·46	20
50	0 47 12·73	36·98	0·30	2 41 30·83	39·61	0·74	4 45 33·18	42·91	0·46	10
13 0	0 47 49·71	36·00	0·31	2 42 10·44	39·62	0·74	4 46 16·09	42·92	0·45	17 0
10	0 48 26·71	37·00	0·31	2 42 50·06	39·64	0·75	4 46 59·01	42·93	0·45	50
20	0 49 3·71	37·00	0·31	2 43 29·70	39·66	0·75	4 47 41·94	42·95	0·44	40
30	0 49 40·71	37·02	0·32	2 44 9·36	39·69	0·75	4 48 24·89	42·96	0·44	30
40	0 50 17·73	37·02	0·32	2 44 49·05	39·70	0·75	4 49 7·85	42·97	0·44	20
50	0 50 54·75	37·04	0·32	2 45 28·75	39·72	0·75	4 49 50·82	42·98	0·43	10
14 0	0 51 31·79	37·04	0·33	2 46 8·47	39·74	0·75	4 50 33·80	43·00	0·43	16 0
10	0 52 8·83	37·04	0·33	2 46 48·21	39·76	0·75	4 51 16·80	43·01	0·43	50
20	0 52 45·87	37·06	0·33	2 47 27·97	39·79	0·75	4 51 59·81	43·02	0·42	40
30	0 53 22·93	37·06	0·34	2 48 7·76	39·80	0·75	4 52 42·83	43·04	0·42	30
40	0 53 59·99	37·08	0·34	2 48 47·56	39·82	0·75	4 53 25·87	43·04	0·41	20
50	0 54 37·07	37·08	0·35	2 49 27·38	39·84	0·75	4 54 8·91	43·06	0·41	10
15 0	0 55 14·15	37·09	0·35	2 50 7·22	39·86	0·75	4 54 51·97	43·07	0·41	15 0
10	0 55 51·24	37·1[illegible]	[illegible]	2 50 47·08	39·88	0·75	4 55 35·04	43·08	0·40	50
20	0 56 28·34	37·10	0·36	2 51 26·96	39·91	0·75	4 56 18·12	43·09	0·40	40
30	0 57 5·44	37·12	0·36	2 52 6·87	39·92	0·75	4 57 1·21	43·10	0·39	30
40	0 57 42·56	37·13	0·36	2 52 46·79	39·94	0·75	4 57 44·31	43·12	0·39	20
50	0 58 19·6[illegible]	37·13	0·37	2 53 26·73	39·96	0·75	4 58 27·43	43·12	0·39	10
16 0	0 58 56·82	37·14	0·37	2 54 6·69	39·98	0·75	4 59 10·55	43·14	0·38	14 0
	Signs V. and XI.			Signs IV. and X.			Signs III. and IX.			

TABLE continued.

Argument.	Signs 0 and VI.			Signs I. and VII.			Signs II. and VIII.			Argument.
⊙'s Long.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	R. A.	Diff.	Sec. Var.	⊙'s Long.
° '	h ' "	"	"	h ' "	"	"	h ' "	"	"	° '
10	0 59 33·96	37·16	0·37	2 54 46·67	40·00	0·75	4 59 53·69	43·15	0·38	50
20	1 0 11·12	37·16	0·38	2 55 26·67	40·02	0·75	5 0 36·84	43·15	0·37	40
30	1 0 48·28	37·17	0·38	2 56 6·69	40·05	0·75	5 1 19·99	43·17	0·37	30
40	1 1 25·45	37·19	0·38	2 56 46·74	40·06	0·75	5 2 3·16	43·18	0·36	20
50	1 2 2·64	37·19	0·39	2 57 26·80	40·08	0·75	5 2 46·34	43·19	0·36	10
17 0	1 2 39·83	37·20	0·39	2 58 6·88	40·10	0·75	5 3 29·53	43·20	0·36	13 0
10	1 3 17·03	37·21	0·40	2 58 46·98	40·13	0·75	5 4 12·73	43·21	0·35	50
20	1 3 54·24	37·22	0·40	2 59 27·11	40·14	0·75	5 4 55·94	43·22	[illegible]	40
30	1 4 31·46	37·23	0·40	3 0 7·25	40·16	0·75	5 5 39·16	43·23	0·34	30
40	1 5 8·69	37·24	0·41	3 0 47·41	40·18	0·75	5 6 22·39	43·24	0·34	20
50	1 5 45·93	37·25	0·41	3 1 27·59	40·21	0·75	5 7 5·63	43·25	0·34	10
18 0	1 6 23·18	37·26	0·41	3 2 7·80	40·22	0·75	5 7 48·88	43·26	0·33	12 0
10	1 7 0·44	37·27	0·42	3 2 48·02	40·25	0·75	5 8 32·14	43·26	0·33	50
20	1 7 37·71	37·29	0·42	3 3 28·27	40·26	0·75	5 9 15·40	43·28	0·32	40
30	1 8 15·00	37·29	0·42	3 4 8·53	40·28	0·75	5 9 58·68	43·29	0·32	30
40	1 8 52·29	37·30	0·43	3 4 48·81	40·31	0·75	5 10 41·97	43·29	0·31	20
50	1 9 29·59	37·32	0·43	3 5 29·12	40·32	0·75	5 11 25·26	43·31	0·31	10
19 0	1 10 6·91	37·32	0·43	3 6 9·44	40·35	0·75	5 12 8·57	43·31	0·31	11 0
10	1 10 44·23	37·33	0·44	3 6 49·79	40·36	0·75	5 12 51·88	43·32	0·30	50
20	1 11 21·56	37·35	0·44	3 7 30·15	40·39	0·75	5 13 35·20	43·34	0·30	40
30	1 11 58·91	37·36	0·44	3 8 10·54	40·40	0·75	5 14 18·54	43·34	0·29	30
40	1 12 36·27	37·37	0·45	3 8 50·94	40·43	0·75	5 15 1·88	43·34	0·29	20
50	1 13 13·64	37·38	0·45	3 9 31·37	40·45	0·75	5 15 45·22	43·36	0·28	10
20 0	1 13 51·02	37·39	0·45	3 10 11·82	40·46	0·75	5 16 28·58	43·36	0·28	10 0
10	1 14 28·41	37·40	0·45	3 10 52·28	40·49	0·75	5 17 11·94	43·37	0·27	50
20	1 15 5·81	37·41	0·46	3 11 32·77	40·50	0·75	5 17 55·31	43·38	0·27	40
30	1 15 43·22	37·43	0·46	3 12 13·27	40·53	0·75	5 18 38·69	43·39	0·27	30
40	1 16 20·65	37·44	0·47	3 12 53·80	40·55	0·75	5 19 22·08	43·40	0·26	20
50	1 16 58·09	37·45	0·47	3 13 34·35	40·57	0·75	5 20 5·48	43·40	0·26	10
21 0	1 17 35·54	37·46	0·47	3 14 14·92	40·59	0·75	5 20 48·88	43·41	0·25	9 0
10	1 18 13·00	37·47	0·48	3 14 55·51	40·60	0·75	5 21 32·29	43·42	0·25	50
20	1 18 50·47	37·48	0·48	3 15 36·11	40·63	0·75	5 22 15·71	43·42	0·24	40
30	1 19 27·95	37·50	0·48	3 16 16·74	40·65	0·75	5 22 59·13	43·43	0·24	30
40	1 20 5·45	37·51	0·48	3 16 57·39	40·67	0·75	5 23 42·56	43·44	0·23	20
50	1 20 42·96	37·52	0·49	3 17 38·06	40·69	0·74	5 24 26·00	43·44	0·23	10
22 0	1 21 20·48	37·53	0·49	3 18 18·75	40·71	0·74	5 25 9·44	43·45	0·23	8 0
10	1 21 58·01	37·55	0·49	3 18 59·46	40·73	0·74	5 25 52·89	43·46	0·22	50
20	1 22 35·56	37·55	0·50	3 19 40·19	40·75	0·74	5 26 36·35	43·46	0·22	40
30	1 23 13·11	37·58	0·50	3 20 20·94	40·77	0·74	5 27 19·81	43·47	0·21	30
40	1 23 50·69	37·58	0·50	3 21 1·71	40·79	0·74	5 28 3·28	43·48	0·21	20
50	1 24 28·27	37·59	0·51	3 21 42·50	40·81	0·74	5 28 46·76	43·48	0·20	10
23 0	1 25 5·86	37·61	0·51	3 22 23·31	40·83	0·74	5 29 30·24	43·49	0·20	7 0
10	1 25 43·47	37·63	0·51	3 23 4·14	40·85	0·74	5 30 13·73	43·49	0·19	50
20	1 26 21·10	37·63	0·52	3 23 44·99	40·87	0·74	5 30 [illegible]	43·50	0·19	40
30	1 26 58·73	37·65	0·52	3 24 25·86	40·89	0·74	5 31 [illegible]	43·51	0·18	30
40	1 27 36·38	37·66	0·52	3 25 6·75	40·91	0·74	5 32 24·23	43·51	0·18	20
50	1 28 14·04	37·67	0·52	3 25 47·66	40·93	0·73	5 33 7·74	43·51	0·18	10
24 0	1 28 51·71	37·69	0·53	3 26 28·59	40·95	0·73	5 33 51·25	43·52	0·17	6 0
	Signs V. and XI.			Signs IV. and X.			Signs III. and IX.			

TABLE continued.

Argument. ⊙'s Long.	Signs 0 and VI. R. A.	Diff.	Sec. Var.	Signs I. and VII. R. A.	Diff.	Sec. Var.	Signs II. and VIII. R. A.	Diff.	Sec. Var.	Argument. ⊙'s Long.
° ′	h ′ ″	″	″	h ′ ″	″	″	h ′ ″	″	″	° ′
10	1 29 29·40	37·70	0·53	3 27 9·54	40·97	0·73	5 34 34·77	43·52	0·17	50
20	1 30 7·10	37·71	0·53	3 27 50·51	41·00	0·73	5 35 18·29	43·53	0·16	40
30	1 30 44·81	37·73	0·54	3 28 31·51	41·01	0·73	5 36 1·82	43·53	0·16	30
40	1 31 22·54	37·74	0·54	3 29 12·52	41·03	0·73	5 36 45·35	43·54	0·15	20
50	1 32 0·28	37·75	0·54	3 29 53·55	41·05	0·73	5 37 28·89	43·54	0·15	10
25 0	1 32 38·03	37·77	0·55	3 30 34·60	41·07	0·73	5 38 12·43	43·55	0·14	5 0
10	1 33 15·80	37·78	0·55	3 31 15·67	41·09	0·73	5 38 55·98	43·55	0·14	50
20	1 33 53·58	37·80	0·55	3 31 56·76	41·11	0·72	5 39 39·53	43·55	0·13	40
30	1 34 31·38	37·81	0·55	3 32 37·87	41·13	0·72	5 40 23·08	43·56	0·13	30
40	1 35 9·19	37·82	0·56	3 33 19·00	41·15	0·72	5 41 6·64	43·56	0·12	20
50	1 35 47·01	37·84	0·56	3 34 0·15	41·17	0·72	5 41 50·20	43·57	0·12	10
26 0	1 36 24·85	37·85	0·56	3 34 41·32	41·19	0·72	5 42 33·77	43·57	0·11	4 0
10	1 37 2·70	37·87	0·56	3 35 22·51	41·21	0·72	5 43 17·34	43·57	0·11	50
20	1 37 40·57	37·88	0·57	3 36 3·72	41·23	0·72	5 44 0·91	43·57	0·10	40
30	1 38 18·45	37·90	0·57	3 36 44·95	41·25	0·72	5 44 44·48	43·58	0·10	30
40	1 38 56·35	37·91	0·57	3 37 26·20	41·27	0·71	5 45 28·06	43·58	0·10	20
50	1 39 34·26	37·92	0·58	3 38 7·47	41·28	0·71	5 46 11·64	43·58	0·09	10
27 0	1 40 12·18	37·94	0·58	3 38 48·75	41·31	0·71	5 46 55·22	43·59	0·09	3 0
10	1 40 50·12	37·95	0·58	3 39 30·06	41·33	0·71	5 47 38·81	43·58	0·08	50
20	1 41 28·07	37·97	0·58	3 40 11·39	41·34	0·71	5 48 22·39	43·59	0·08	40
30	1 42 6·04	37·98	0·59	3 40 52·73	41·37	0·71	5 49 5·98	43·59	0·07	30
40	1 42 44·02	38·00	0·59	3 41 34·10	41·38	0·70	5 49 49·57	43·60	0·07	20
50	1 43 22·02	38·02	0·59	3 42 15·48	41·41	0·70	5 50 33·17	43·59	0·06	10
28 0	1 44 0·04	38·02	0·59	3 42 56·89	41·43	0·70	5 51 16·76	43·60	0·06	2 0
10	1 44 38·06	38·05	0·60	3 43 38·32	41·44	0·70	5 52 0·36	43·60	0·05	50
20	1 45 16·11	38·06	0·60	3 44 19·76	41·46	0·70	5 52 43·96	43·60	0·05	40
30	1 45 54·17	38·07	0·60	3 45 1·22	41·48	0·70	5 53 27·56	43·60	0·04	30
40	1 46 32·24	38·09	0·60	3 45 42·70	41·51	0·69	5 54 11·16	43·60	0·04	20
50	1 47 10·33	38·11	0·61	3 46 24·21	41·52	0·69	5 54 54·76	43·61	0·03	10
29 0	1 47 48·44	38·11	0·61	3 47 5·73	41·54	0·69	5 55 38·37	43·60	0·03	1 0
10	1 48 26·55	38·14	0·61	3 47 47·27	41·56	0·69	5 56 21·97	43·61	0·02	50
20	1 49 4·69	38·15	0·61	3 48 28·83	41·57	0·69	5 57 5·58	43·60	0·02	40
30	1 49 42·84	38·17	0·62	3 49 10·40	41·60	0·69	5 57 49·18	43·61	0·01	30
40	1 50 21·01	38·18	0·62	3 49 52·00	41·62	0·68	5 58 32·79	43·60	0·01	20
50	1 50 59·19	38·20	0·62	3 50 33·62	41·63	0·68	5 59 16·39	43·61	0·00	10
30 0	1 51 37·39		0·62	3 51 15·25		0·68	6 0 0·00		0·00	0 0
	Signs V. and XI.			Signs IV. and X.			Signs III. and IX.			

N. B. If the ⊙'s longitude is between 0 and III. signs, the Table gives the R. A. If between III. and VI. signs, subtract the R. A. as given by the Table from 12 hours, the remainder is the R. A. If between VI. and IX. signs, add 12 hours to the R. A. But if between IX. and XII. signs, subtract the result given by the Table from 24 hours, and the remainder is the R. A.

The secular equation of the ⊙'s R. A. is additive if subsequent to 1801, otherwise it is to be subtracted from the R. A. as given by the Table according with the above directions.

XXIX. *On Sounds inaudible by certain Ears.* By WILLIAM HYDE WOLLASTON, *M.D. P.R.S.**

IT is not my intention to occupy the time of this Society with the consideration of that mere general dullness to the impression of all kinds of sound which constitutes ordinary deafness, but to request its attention to certain peculiarities that I have observed with respect to partial insensibility in different states of the ear, and in different individuals; for I have found that an ear, which would be considered as perfect with regard to the generality of sounds, may, at the same time, be completely insensible to such as are at one or the other extremity of the scale of musical notes, the hearing or not hearing of which seems to depend wholly on the pitch or frequency of vibration constituting the note, and not upon the intensity or loudness of the noise.

Indeed, although persons labouring under common deafness have an imperfect perception of all sounds, the degree of indistinctness of different sounds is commonly not the same; for it will be found upon examination, that they usually hear sharp sounds much better than low ones; they distinguish the voices of women and children better than the deeper tones in which men commonly speak; and it may be remarked, that the generality of persons accustomed to speak to those who are deaf, seem practically aware of this difference, and, even without reflecting upon the motives which guide them, acquire a habit of speaking to deaf persons in a shriller tone of voice, as a method by which they succeed in making them hear more effectually than by merely speaking louder.

In elucidation of this state of hearing, which casually occurs as a malady, I have observed, that other ears may for a time be reduced to the same condition of insensibility to low sounds. I was originally led to this observation, in endeavouring to investigate the cause of deafness in a friend, by trial of different modes of closing, or otherwise lessening the sensibility of my own ears. I remarked that, when the mouth and nose are shut, the tympanum may be so exhausted by forcible attempt to take breath by expansion of the chest, that the pressure of the external air is strongly felt upon the membrana tympani, and that, in this state of tension from external pressure, the ear becomes insensible to grave tones, without losing in any degree the perception of sharper sounds.

The state to which the ear is thus reduced by exhaustion, may even be preserved for a certain time without the continued effort of inspiration, and without even stopping the breath, since by

* From the Transactions of the Royal Society for 1820, Part II.

 sudden

sudden cessation of the effort, the internal passage to the ear becomes closed by the flexibility of the Eustachian tube, which acts as a valve, and prevents the return of air into the tympanum. As the defect thus occasioned is voluntary, so also is the remedy; for the unpleasant sensation of pressure on the drum, and the partial deafness which accompanies it, may at any instant be removed by the act of swallowing, which opens the tube, and, by allowing the air to enter, restores the equilibrium of pressure necessary to the due performance of the functions of the ear.

In my endeavours to ascertain the extent to which this kind of deafness may be carried, some doubt has arisen, from the difficulty of finding sounds sufficiently pure for the purpose. The sounds of stringed instruments are in this respect defective; for unless the notes produced are free from any intermixture of their sharper chords, some degree of deception is very liable to occur in the estimate of the lowest note really heard. I can, nevertheless, with considerable confidence, say, that my own ears may be rendered insensible to all sounds below F marked by the base cliff. But as I have been in the habit of making the experiment frequently, it is probable that other persons who may be inclined to repeat it, will not with equal facility effect so high a degree of exhaustion as I have done. To a moderate extent the experiment is not difficult, and well worth making. The effect is singularly striking, and may aptly be compared to the mechanical separation of larger and smaller bodies by a sieve. If I strike the table before me with the end of my finger, the whole board sounds with a deep dull note. If I strike it with my nail, there is also at the same time a sharp sound produced by quicker vibrations of parts around the point of contact. When the ear is exhausted it hears only the latter sound, without perceiving in any degree the deeper note of the whole table. In the same manner, in listening to the sound of a carriage, the deeper rumbling noise of the body is no longer heard by an exhausted ear; but the rattle of a chain or loose screw remains at least as audible as before exhaustion.

Although I cannot propose such an experiment as a means of improving the effect of good music, yet, as a source of amusement even from a defective performance, I have occasionally tried it at a concert with singular effect; since none of the sharper sounds are lost, but by the suppression of a great mass of louder sounds, the shriller ones are so much the more distinctly perceived, even to the rattling of the keys of a bad instrument, or scraping of catgut unskilfully touched.

Those who attempt exhaustion of the ear for the first time, rarely have any difficulty in making themselves sensible of external pressure on the tympanum; but it is not easy at first to relax

relax the effort of inspiration with sufficient suddenness to close the Eustachian tube, and thus maintain the exhaustion; neither is it very easy to refrain long together from swallowing the saliva, which instantly puts an end to the experiment.

I may here remark, that this state of excessive tension of the tympanum is sometimes produced by sudden increase of external pressure, as well as by decrease of that within, as is often felt in the diving-bell as soon as it touches the water; the pressure of which upon the included air closes the Eustachian tube, and, in proportion to the descent, occasions a degree of tension on the tympanum, that becomes distressing to persons who have not learned to obviate this inconvenience. Those who are accustomed to descend, probably acquire the art of opening the Eustachian tube by swallowing, or incipient yawning, as soon as the diving-bell touches the water.

It seems highly probable that, in the state of artificial tension thus produced, a corresponding deafness to low tones is occasioned; but, as I never have been in that situation, I have not had an opportunity of ascertaining this point by direct experiment.

In the natural healthy state of the human ear, there does not seem to be any strict limit to our power of discerning low sounds. In listening to those pulsatory vibrations of the air of which sounds consists, if they become less and less frequent, we may doubt at what point tones suited to produce any musical effect terminate; yet all persons but those whose organs are palpably defective continue sensible of vibratory motion, until it becomes a mere tremor, which may be felt and even almost counted.

On the contrary, if we turn our attention to the opposite extremity of the scale of audible sounds, and with a series of pipes exceeding each other in sharpness, if we examine the effects of them successively upon the ears of any considerable number of persons, we shall find (even within the range of those tones which are produced for their musical effects) a very distinct and striking difference between the powers of different individuals, whose organs of hearing are in other respects perfect, and shall have reason to infer, that human hearing in general is more confined than has been supposed with regard to its perception of very acute sounds, and has probably, in every instance, some definite limit, at no great distance beyond the sounds ordinarily heard.

It is now some years since I first had occasion to notice this species of partial deafness, which I at that time supposed to be peculiar to the individual in whom I observed it. While I was endeavouring to estimate the pitch of certain sharp sounds, I remarked

remarked in one of my friends a total insensibility to the sound of a small organ pipe, which, in respect to acuteness, was far within the limits of my own hearing, as well as of others of our acquaintance. By subsequent examination, we found that his sense of hearing terminated at a note four octaves above the middle E of the piano-forte. This note he seemed to hear rather imperfectly, but he could not hear the F next above it, although his hearing is in other respects as perfect, and his perception of musical pitch as correct, as that of any ordinary ears.

The casual observation of this peculiarity in the organ of hearing, soon brought to my recollection a similar incapacity in a near relation of my own, whom I very well remember to have said, when I was a boy, that she never could hear the chirping that commonly occurs in hedges during a summer's evening, which I believe to be that of the *gryllus campestris.*

I have reason to think that a sister of the person last alluded to had the same peculiarity of hearing, although neither of them was in any degree deaf to common sounds.

The next case which came to my knowledge was in some degree more remarkable, in as much as the deafness in all probability extended a note or two lower than in the first instance. This information is derived from two ladies of my acquaintance, who agree that their father could never hear the chirping of the common house sparrow. This is the lowest limit to acute hearing that I have met with, and I believe it to be extremely rare. Deafness even to the chirping of the house cricket, which is several notes higher, is not common. Inability to hear the piercing squeak of the bat seems not very rare, as I have met with several instances of persons not aware of such a sound. The chirping, which I suppose to be that of the *gryllus campestris,* appears to be rather higher than that of the bat, and accordingly will approach the limit of a greater number of ears; for, as far as I am yet able to estimate, human hearing in general extends but a few notes above this pitch. I cannot, however, measure these sounds with precision; for it is difficult to make a pipe to sound such notes, and still more difficult to appreciate the degree of their acuteness.

The chirping of the sparrow will vary somewhat in its pitch, but seems to be about four octaves above E in the middle of the piano-forte.

The note of the bat may be stated at a full octave higher than the sparrow, and I believe that some insects may reach as far as one octave more; for there are sounds decidedly higher than that of a small pipe one-fourth of an inch in length, which cannot be far from six octaves above the middle E. But since this pipe is at the limit of my own hearing, I cannot judge how much the

note

note to which I allude might exceed it in acuteness, as my knowledge of the existence of this sound is derived wholly from some young friends who were present, and heard a chirping, when I was not aware of any sound. I suppose it to have been the cry of some species of *gryllus*, and I imagine it to differ from the *gryllus campestris*, because I have often heard the cry of that insect perfectly.

From the numerous instances in which I have now witnessed the limit to acuteness of hearing, and from the distinct succession of steps that I might enumerate in the hearing of different friends, as the result of various trials that I have made among them, I am inclined to think, that at the limit of hearing, the interval of a single note between two sounds may be sufficient to render the higher note inaudible, although the lower note is heard distinctly.

The suddenness of the transition from perfect hearing to total want of perception, occasions a degree of surprise, which renders an experiment on this subject with a series of small pipes among several persons rather amusing. It is curious to observe the change of feeling manifested by various individuals of the party, in succession, as the sounds approach and pass the limits of their hearing. Those who enjoy a temporary triumph, are often compelled, in their turn, to acknowledge to how short a distance their little superiority extends.

Though it has not yet occurred to me to observe a limit to the hearing of sharp sound in any person under twenty years of age, I am persuaded, by the account that I have received from others, that the youngest ears are liable to the same kind of insensibility. I have conversed with more than one person who never heard the cricket or the bat; and it appears far more likely that such sounds were always beyond their powers of perception, than that they never had been uttered in their presence.

The range of human hearing comprised between the lowest notes of the organ and the highest known cry of insects, includes more than nine octaves, the whole of which are distinctly perceptible by most ears, although the vibrations of a note at the higher extreme are six or seven hundred fold more frequent than those which constitute the gravest audible sound.

Since there is nothing in the constitution of the atmosphere to prevent the existence of vibrations incomparably more frequent than any of which we are conscious, we may imagine that animals like the *grylli*, whose powers appear to commence nearly where ours terminate, may have the faculty of hearing still sharper sounds, which at present we do not know to exist; and that there may be other insects hearing nothing in common with us, but endued with a power of exciting, and a sense that perceives vibrations

tions of the same nature indeed as those which constitute our ordinary sounds, but so remote, that the animals who perceive them may be said to possess another sense, agreeing with our own solely in the medium by which it is excited, and possibly wholly unaffected by those slower vibrations of which we are sensible.

I should be always most unwilling to occupy the time of this Society with idle speculations on mere possible modes of existence, and should not have called its attention to this subject, had I not observed several curious facts which I thought might prove interesting, and may serve to justify some latitude of conjecture beyond the strict evidence of our senses.

XXX. *On the Constitution of aqueous Ammonia.* *By* Andrew Ure, *M.D. Professor of the Andersonian Institution, &c.*

In the article Ammonia of the Dictionary of Chemistry, lately published, after giving the valuable table of Sir H. Davy, I have said, "Probably the quantity of ammonia stated in the above table is too high by about 1 per cent." This remark was hazarded in consequence of some results which I obtained, in a train of experiments which I instituted four years ago, with the view of constructing new tables of the aqueous solutions of the alkalies, earths, and most important neutral salts.

The difference between Mr. Dalton's table of aqueous ammonia, adopted by Dr. Thomson in the last two editions of his System, and the tables of Sir H. Davy and my own, induced me lately to perform the following experiments, the results of which will, I hope, prove acceptable to practical chemists.

I impregnated distilled water with pure ammoniacal gas, till its specific gravity at 60° was found to be exactly 0·900. 500 grains of this, mixed with 500 grains of distilled water, and agitated in a close phial, gave, after a proper interval to ensure intimate combination, a specific gravity at 60° F. of 0·94550
While the mean sp. gr. of the components is .. .. 0·94737

500 grains of this diluted water of ammonia being combined with 500 grains of distilled water, formed a compound having a specific gravity of .. .. 0·97130
While the mean sp. gr. of the components is .. .. 0·97297

Hence we see that the remarkable expansiveness, which ammonia carries into its first condensation with water, continues in the subsequent dilutions of its aqueous combinations. This curious property is not peculiar to pure ammonia, but belongs, I have found, to some of its salts. Thus sal ammoniac, by its union with water, causes an enlargement of the total volume of the

the compound, beyond the volume of the constituents of the solution. Or the specific gravity of the saturated solution is *less*, than the mean sp. gr. of the salt and water. I know of no salts, with which this phænomenon occurs, except the ammoniacal.

From the above experiments we may infer, that when Mr. Dalton affirmed that the progressive dilutions of water of ammonia are effected without changing the mean volume or density of the compound, he had been led into the mistake, probably by the imperfection of his instruments for taking specific gravity.

In endeavouring to ascertain the quantity of real ammonia in the aqueous compounds, I took the most diluted of the above waters, to prevent the chance of any ammonia being exhaled during the experiments.

(1). 100 grains of the ammoniacal water, sp. gr. 0·9713, containing 25 grains of the strongest, required for neutralization, of dilute sulphuric acid, a quantity equivalent to 17·7 grains of distilled and thoroughly concentrated oil of vitriol. Hence we have this proportion in the system of *prime* equivalents,

6·125 : 2·125 : : 17·7 : 6·14, which number multiplied by 4, gives 24·56, for the real ammonia, in 100 by weight of the strongest water.

After careful evaporation in a platina capsule, 24·3 grains of sulphate of ammonia were obtained. My former researches on the ammoniacal salts, published in 1817, showed that this desiccated salt retained one prime equivalent of water. Hence we say

8·25 : 2·125 : : 24·3 : 6·25, which multiplied by 4, gives 25 for the grains of ammonia in 100 of the water at 0·900.

(2). 100 grains of the water of 0·9713 were saturated with a quantity of dilute muriatic acid, equivalent, by my table in the Dictionary, to 13·5 grains of chlorine; and yielded after cautious evaporation, in a platina capsule, 20·3 grains of dry sal ammoniac. From the saturation-experiment, we have this proportion:

4·5 : 2·125 : : 13·5 : 6·375, which multiplied by 4, gives 25·5 for the ammonia in 100 gr. of the water of 0·900. From the saline product we obtain this proportion:

6·75 : 2·125 : : 20·3 : 6·39; and $6·39 \times 4 = 25·56 =$ the ammonia in 100 of the water of 0·900.

(3). 100 grains of the same dilute ammonia, being saturated with nitric acid, yielded 28·7 grains of dry nitrate. But I have formerly shown that the desiccated nitrate retains a prime equivalent of water. Hence

$10 (= 6·75 + 2·125 + 1·125) : 2·125 : : 28·7 : 6·1$; and $6·1 \times 4 = 24·4$, a quantity somewhat less than the preceding results, as

 might

might be expected from the greater volatility and *decomposability* of this salt; but still according sufficiently well with the above equivalent (or atomic) constitution.

The experiments with the sulphuric and muriatic acids were several times repeated, so that I have no doubt but we may safely estimate the real alkali in 100 grains of the water of ammonia of 0·900, to be not less than 25 grains. Sir H. Davy makes it 26, and Mr. Dalton 22·2.

I am preparing a complete table for every density intermediate between 0·900 and 1·000. Meanwhile the following rule for ascertaining the proportion of ammonia, will be found convenient and tolerably correct. *Subtract the number denoting the specific gravity of the aqueous ammonia, expressed in three integers, from 997, and divide the remainder by 4; the quotient will be the per centage of alkali.*

EXAMPLES. Thus, the density is 945·5. What is the per centage of ammonia?

$997 - 945{\cdot}5 = 51{\cdot}5$; and $\frac{51{\cdot}5}{4} = 12{\cdot}875$ = the ammonia in 100 grains of the water.

The specific gravity is 971·3. The proportion of the constituents is sought.

$997 - 971{\cdot}3 = 25{\cdot}7$; and $\frac{25{\cdot}7}{4} = 6{\cdot}425$, for the ammonia; and of course 93·575, for the water present. Both these determinations are, I believe, very near the proportions as given by experiment.

Since 75 of water become, in water of ammonia of 0·900, 100 by weight, and 111·11 by measure; it is evident, that 6·75 in bulk of water, become 10 in bulk of that aqueous ammonia; for 6·75 : 10 : : 75 : 111·11. A slight typographical error requires to be corrected in the second column of the article AMMONIA of the Dictionary. "Correcting the first error, where 6 is substituted for 9," should be read, "where 6 is substituted for 6·75."

If we reckon 25 to be the true per centage of alkali in the above water; and the weight of 100 cubic inches of the gas to be 18 grains, then 75 grains of water combine with 138·88 cubic inches of gas ($= \frac{100 \times 25}{18}$); or 1 grain of water combines with 1·852 cubic inches, equivalent to 467·63 grain measures; ($= 1{\cdot}852 \times 252{\cdot}5$). Thus 1 volume of water condenses 467·63 volumes of gas, and becomes 1·48 in volume of aqueous ammonia of 0·900. And if these 1·48 volumes contain 467·63 of gas, 1 volume of the water will contain 316 of gas.

We

We thus see that the statement which Dr. Thomson gave in his 5th edition, and which is repeated in the 6th, is still wider of the truth than I have represented it in the Dictionary. He says, Water condenses 780 times its volume of the gas, and becomes of specific gravity 0·900. I allowed him inadvertently two-thirds of the quantity, or 520 volumes, which is too much. At the head of the 3d column, for "520 volumes," it should have been 443 volumes. It is also said, "Sir H. Davy's table differs very little from that of Mr. Dalton, the truth probably lying between them." Now, if we consider the enormous error of Dr. Thomson's number 36 per cent., corresponding to his 780 volumes; Mr. Dalton's number 22·2 is but an inconsiderable deviation from Sir H. Davy's number 26, which is undoubtedly very near the truth.

Glasgow, Feb. 20, 1821.

Glasgow, March 21, 1821.

Since writing the preceding account, the prosecution of my new system of chemical analysis, announced in the introduction to the Dictionary, of which you have been pleased to give so kind a report, has led me to construct the following table of the aqueous combinations of ammonia. The experiments, on which it is founded, were conducted with every refinement of precision which I could think of.

It will be observed that the initial quantity, 26·5, or the real ammonia in water of density 0·900, is somewhat greater than that formerly deduced. This increase is owing to the prevention of exhalation of ammonia during the process of experimenting; a pretty dilute water being employed, and its exact neutralization with acid being rapidly accomplished. If, on the other hand, one hundred grains of water, whose density is 0·9455, be exposed during its neutralization, in a capsule for a few minutes, half a grain of ammoniacal gas may escape, which will cause a deficiency of one per cent. in the estimate of alkali in water of 0·900. The terms marked with the asterisk are experimental: the others are interpolated. The error in any of them cannot amount, I believe, to more than a small fraction of one per cent.

Table of the Quantity of Ammonia in 100 *Parts, by Weight, of its aqueous Combinations at successive Densities.*

	Ammonia in 100.	Water in 100.	Specific Gravity by Experiment.	Computed or mean Sp. Grav.	Equivalent Primes of Water to 1 of Ammonia.
	27·940	72·060	0·8914*		
	27·633	72·367	0·8937*		
of	27·038	72·962	0·8967*		27·4 to 72·6; 1 to 5
0·900	26·751	73·249	0·8983*		
100	26·500	73·500	0·9000*		
95	25·175	74·825	0·9045*	0·90452	
90	23·850	76·150	0·9090*	0·90909	24 to 76; 1 to 6
85	22·525	77·475	0·9133	0·91370	
80	21·200	78·800	0·9177*	0·91838	21·25 to 78·75; 1 to 7
75	19·875	80·125	0·9227	0·92308	
70	18·550	81·450	0·9275*	0·92780	19·1 to 80·9; 1 to 8
65	17·225	82·775	0·9320	0·93264	17·35 to 82·65; 1 to 9
60	15·900	84·100	0·9363*	0·93750	15·9 to 84·1; 1 to 10
55	14·575	85·425	0·9410	0·94241	14·66 to 85·34; 1 to 11
50	13·250	86·750	0·9455*	0·94737	13·60 to 86·40; 1 to 12
45	11·925	88·075	0·9510	0·95238	11·9 to 88·1; 1 to 14
40	10·600	89·400	0·9564*	0·95744	11·2 to 88·8; 1 to 15
35	9·275	90·725	0.9614	0·96256	
30	7·950	92·050	0·9662*	0·96774	8·63 to 91·37; 1 to 20
25	6·625	93·375	0·9716	0·97297	7· to 93· 1 to 25
20	5·300	94·700	0·9768*	0·97826	6 to 94 1 to 30
15	3·975	96·025	0·9828	0·98360	4·5 to 95·5; 1 to 40
10	2·650	97·350	0·9887*	0·9890	3 to 97; 1 to 60
5	1·325	98·675	0·9945	0·99447	

Near the two extremities of the table, the experimental and computed specific gravities agree; the reciprocal affinity then balancing the peculiar expansiveness communicated by the ammonia, which becomes conspicuous in the intermediate proportions of water and gas. This fact is in unison with the general laws of chemical combination.

The agreement between the experimental points given in Sir H. Davy's table, and the corresponding ones of the above, is very satisfactory, and must consequently inspire confidence in my results.

Since 73·5 grains of distilled water exist in 100 of water of ammonia, specific gravity 0·900, which occupy the volume of 1·111; one part of water in bulk will be converted into almost exactly

exactly one and a half of such water of ammonia. 100 grains of this water contain 147·2 cubic inches of gas. Hence one *grain* of water holds condensed in such aqueous ammonia, two cubic inches of the gas; or one *volume* of distilled water is united to 505 volumes of gas.

It is a remarkable coincidence, that one volume of water when converted into aqueous muriatic acid, specific gravity 1·200; or into aqueous ammonia, specific gravity 0.900, expands in either case into a volume and a half.

If from 998 we deduct the specific gravity of water of ammonia, expressed in three integers, the remainder, divided by 4, will give for a quotient the quantity of alkali contained in all such liquid ammonia as is used in medicine, or in chemical researches, viz. between specific gravities 0·936 and 0·980.

XXXI. *Right Ascension, Declination, and Passage of the Meridian of Ceres; and true apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from p. 133.]

Right Ascension, &c. of Ceres.

1821.	Rt. Asc.	Declin.	Passage of the Meridian.
May	H. M.	° ′	H. M.
1	16 23	14 48 S.	13 49
7	16 18	14 51	13 15
13	16 13	14 54	12 56
19	16 7	14 57	12 23
25	15 58	15 2	11 51
June 1	15 52	15 11 S.	11 14
7	15 46	15 20	11 40
13	15 42	15 30	10 6
19	15 38	15 42	10 42
25	15 35	15 56	9 19

Table

1821.	γ Pegasi.	α Arietis	α Ceti.	Aldebaran	Capella	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 14	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
May	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	2·13	6·26	56·04	39·91	29·26	56·63	59·60	29·65	15·88	11·20	56·74	22·41	48·92	51·82	57·88	24·71	49·02	32·85
2	15	28	05	91	25	62	59	64	87	18	73	39	90	81	87	70	02	86
3	17	29	06	91	25	62	59	64	86	17	72	38	89	80	87	70	03	87
4	20	30	07	91	24	61	58	63	85	16	71	37	87	79	86	69	03	87
5	22	31	08	91	24	61	58	62	84	15	70	36	86	78	85	68	03	87
6	25	33	09	91	24	61	58	61	83	13	69	35	85	77	84	67	03	88
7	27	34	10	91	24	60	58	60	82	12	68	34	83	76	83	67	03	88
8	30	35	10	91	23	60	58	60	81	11	67	33	82	75	83	66	04	88
9	32	37	11	92	23	60	58	60	80	10	66	32	81	74	82	66	04	89
10	34	39	12	92	23	60	58	59	79	09	65	31	80	73	81	65	04	89
11	37	41	13	92	23	60	58	59	78	08	64	30	79	72	81	65	04	89
12	40	43	15	93	23	60	58	58	77	07	63	29	78	71	80	64	04	89
13	42	45	17	93	23	60	58	58	77	06	62	28	76	69	79	64	04	89
14	45	47	18	94	23	60	58	58	76	05	61	27	75	68	78	63	04	89
15	48	49	19	94	23	60	58	58	75	04	61	26	74	67	78	62	04	89
16	51	51	21	95	24	60	58	58	74	03	60	25	73	66	77	61	04	90
17	53	53	22	95	24	60	58	58	74	02	59	24	72	65	76	60	04	90
18	56	55	24	96	24	60	58	58	73	01	58	23	71	64	76	60	04	90
19	60	58	25	97	24	60	58	57	72	10·00	58	22	70	63	75	59	03	90
20	62	60	27	98	24	60	58	57	71	99	57	21	69	62	74	58	·03	90
21	65	62	28	99	25	60	58	57	70	98	56	20	68	61	73	57	03	90
22	67	65	30	40·00	25	61	59	57	70	98	56	19	67	60	72	56	02	90
23	70	67	32	01	26	61	59	58	70	97	55	19	65	59	71	55	02	90
24	73	69	34	02	27	62	60	58	69	97	55	18	64	58	70	54	01	90
25	76	71	35	02	28	62	60	58	69	96	54	17	63	57	70	54	01	90
26	79	74	37	03	29	62	61	58	69	96	54	16	62	56	69	53	01	89
27	82	76	39	04	30	63	61	59	68	96	54	16	61	55	68	53	48·00	89
28	85	78	41	05	31	63	62	59	68	95	53	16	60	54	67	52	00	89
29	88	81	43	06	32	64	63	59	67	95	53	15	59	53	66	51	00	88
30	91	83	45	07	33	64	64	60	67	94	53	15	58	52	65	50	99	88
31	94	86	47	08	34	65	65	60	67	94	53	15	57	51	64	50	98	88

1821.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri	α Cygni	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
May	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50·93	2·37	9·55	30·18	28·78	31·87	40·14	54·68	46·73	4·67	32·97	45·01	8·76	20·77	36·36	45·39	51·69	9·40
2	94	38	56	20	80	89	17	71	76	70	33·00	04	79	80	39	42	72	43
3	95	39	58	21	82	91	19	74	79	73	03	08	83	84	42	45	75	45
4	96	40	59	22	84	93	22	77	82	76	06	11	86	87	45	48	78	48
5	97	41	60	23	86	95	24	79	84	79	08	14	89	91	48	51	81	50
6	98	42	62	24	88	97	26	82	87	82	11	17	92	94	51	55	84	53
7	99	43	63	25	90	99	29	85	90	85	14	20	95	98	54	58	87	55
8	51·00	44	64	27	92	32·01	31	88	93	88	17	24	98	21·02	57	62	90	58
9	01	45	65	28	94	03	34	91	96	91	20	27	9·02	05	60	65	93	61
10	02	46	66	29	96	05	36	94	99	94	23	30	05	09	63	68	96	64
11	03	47	67	30	98	07	38	97	02	97	26	33	08	13	66	71	99	67
12	03	47	68	31	99	09	41	55·00	05	5·00	29	36	11	17	69	75	52·02	70
13	04	48	69	32	29·01	11	43	02	08	03	32	40	15	21	72	78	05	73
14	04	48	70	33	03	13	45	05	11	06	35	43	18	24	75	81	08	76
15	05	49	71	34	05	15	47	07	14	09	38	46	21	28	79	85	11	79
16	05	49	72	35	07	17	49	10	18	12	41	50	24	32	82	88	14	82
17	06	50	73	36	08	19	51	12	20	15	44	52	27	35	86	92	17	85
18	06	50	73	37	10	21	53	15	23	18	47	55	30	39	89	95	20	88
19	07	51	74	38	11	22	55	17	25	21	50	58	33	42	91	99	23	91
20	07	51	74	39	13	24	57	20	28	24	53	61	36	46	94	46·02	26	94
21	08	52	75	40	14	25	59	22	31	27	55	64	39	49	97	06	29	97
22	09	53	76	41	15	27	60	24	33	29	58	67	42	53	37·00	09	32	10·00
23	10	54	76	42	17	28	62	27	36	32	61	70	45	56	03	12	35	03
24	10	54	77	43	18	30	63	29	38	34	64	73	48	59	06	16	38	06
25	10	54	77	43	19	31	65	31	41	37	66	75	50	62	09	19	41	09
26	11	55	78	43	20	32	67	33	44	40	69	78	53	66	12	22	44	12
27	11	55	78	44	21	34	68	35	46	42	72	81	56	69	15	26	47	16
28	11	55	79	45	23	35	70	38	49	45	75	84	59	72	18	29	50	19
29	12	56	79	46	24	37	71	40	51	47	77	87	62	76	21	33	53	22
30	12	56	80	47	25	38	73	42	54	50	80	90	65	79	24	36	57	25
31	12	56	80	47	27	40	74	44	56	52	82	93	68	83	28	40	60	28

XXXII. *Some Account of a Method which may be applied to the same Purposes as Sir* Isaac Newton's *Method of Fluxions. By Mr.* Thomas Tredgold.

Letter II.

To Mr. Tilloch.

Sir, — In my first letter (see p. 177 of this Number) on this subject, I explained a method of exhibiting the areas and the lengths of curves by means of progressions. I intend to show how the same principle may be applied to other parts of the method of fluxions; but, before I proceed further, it will be necessary to show how such progressions may be summed: not that the problem has not been previously treated, but only because I follow a method of my own, which, perhaps, has advantages not common to other methods.

Assume a progression of the following form, and range the first differences of the adjoining terms below it: thus

$$0 + a^n + (a+d)^n + (a+2d)^n + \ldots (a+[m-1]d)^n + (a+md)^n.$$

$$a^n, (a+d)^n - a^n, (a+2d)^n - (a+d)^n, \ldots (a+md)^n - (a+[m-1]d)^n.$$

Then, it is easily proved that the last term of the progression is equal to the sum of all the differences. Also, if the second differences be taken, the last term of the first differences is equal to the sum of the second differences; and so of any other order of differences.

For example: If $n = 1$, the differences will be $a, d, d, d,$ &c. and if S be taken to represent the sum of these differences, $S = a + md$.

Also, if $n = 2$, then the differences will become

$$a, 2ad + d^2, 2ad + 3d^2, \ldots \ldots 2ad + (2m-1)d^2.$$
and their sum $S = (a+md)^2$.

When $a = 0$ the progression becomes extremely simple, and under this form I intend to show its application.

$$\text{Prog. } 0 + d^n + (2d)^n + (3d)^n + \ldots \ldots ([m-1]d)^n + (md)^n.$$

$$\text{Diff}^s.\ d^n, (2d)^n - d^n, (3d)^n - (2d^n, \ldots \ldots (md)^n - ([m-1]d)^n.$$

By means of this progression, any other which increases or decreases by the same law as the differences may be summed. This is effected by comparing the last term of the proposed one with the last difference of this progression: and if both be expressed by the same power or powers of m, with constant quantities; it only remains to determine the value of d, which is a constant

constant quantity introduced in the above progression for the purpose of rendering it capable of being compared with progressions into which constant quantities enter.

As an example I shall take the progression

$$\frac{ax^{n+1}}{2m^{n+1}} \times \left(1+(1+2^n)+(2^n+3^n)+ \ldots\ldots (m-1)^n+m^n\right)$$

which, I have shown in my First Letter, will express the area of a curve of which the ordinate is ax^n and the abscissa x.

The last term of this progression is $\frac{ax^{n+1}}{2m^{n+1}} \times \left(\overline{m-1}^n+m^n\right)$.

And to make the last difference of the assumed progression contain the same powers of m, when reduced, it must be

$$(md)^{n+1}-\left([m-1]d\right)^{n+1}=d^{n+1}\times\left(m^{n+1}-[m-1]^{n+1}\right).$$

Making

$$\frac{ax^{n+1}}{2m^{n+1}}\times\left([m-1]^n+m^n\right)=d^{n+1}\times\left(m^{n+1}-[m-1]^{n+1}\right),$$ we find $d^{n+1}=\frac{ax^{n+1}}{2m^{n+1}}\times\left(\frac{[m-1]^n+m^n}{m^{n+1}-[m-1]^{n+1}}\right)$. But the sum of the progression will be $(md)^{n+1}=\frac{ax^{n+1}}{2}\times\left(\frac{m^n+[m-1]^n}{m^{n+1}-[m-1]^{n+1}}\right)$.

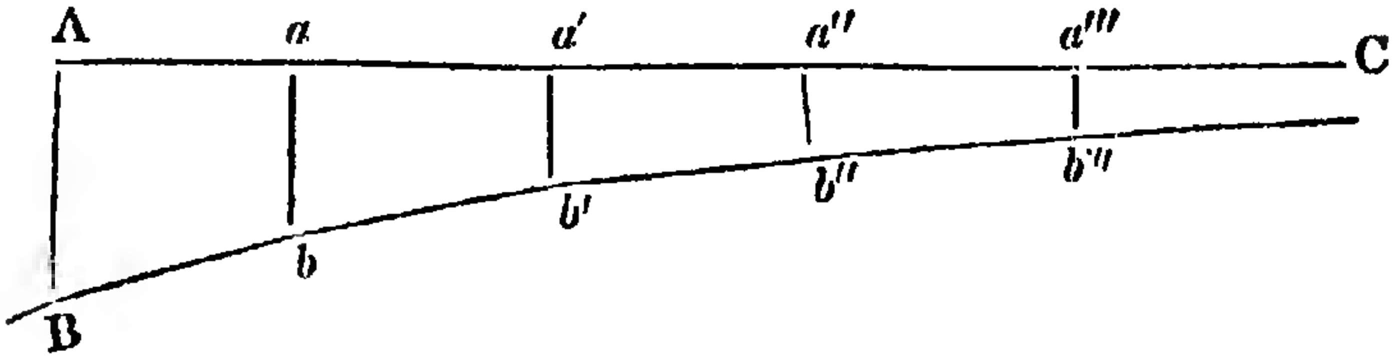
If the numerator and denominator of the latter part of this expression be expanded by means of the binomial theorem, we obtain the sum of the progression under the form

$$\frac{ax^{n+1}}{n+1}\times\left\{\frac{1-nm^{-1}+n\frac{n-1}{2}m^{-2}-n\frac{n-1}{2}\,\frac{n-2}{3}m^{-3}+\&c.}{1-nm^{-1}+n\frac{n-1}{3}m^{-2}-n\frac{n-1}{3}\cdot\frac{n-2}{4}m^{-3}+\&c.}\right\}.$$

It will readily be seen that the sum of the progression can never be accurately represented by $\frac{ax^{n+1}}{n+1}$ except when $n=1$; but it is evidently nearer to the true sum in proportion as the value of m is increased.

Let AC be a straight line, and make AB, ab, $a'b'$, $a''b''$, &c. each perpendicular to it; then, if AB, ab, $a'b'$, &c. represent the successive differences between $\frac{ax^{n+1}}{n+1}$ and the true sum of the progression corresponding to certain increased values of m, and if a line be drawn through the points B, b, b', &c. it will continually approach the line AC, as a curve does to its asymptote; but

but so long as m is any imaginable number these lines will not meet. It is therefore clear that we cannot express the area of



a curvilinear space accurately by $\frac{ax^{n+1}}{n+1}$ if it depends upon the sum of the progression being accurately given by the same expression. But it is obvious that in proportion as we increase the value of m, the progression approaches nearer to the true area of the space, because $\frac{x}{m}$ becomes less; and if we suppose this approach to accuracy to be measured by a deflexion of the line AC towards B b''', the two lines must ultimately meet. And as we can always suppose the value of m to be such that they would meet; $\frac{ax^{n+1}}{n+1}$ must truly express the area.

The truth of the result is a consequence of the causes of error neutralising one another, and which necessarily flows from the method of investigation. The result is essentially the same as is obtained by the method of fluxions; indeed it is one of the most important of the rules of that celebrated method investigated by common processes. I much suspect that neither Sir Isaac Newton nor Leibnitz ever understood the real nature of the method of fluxions; if this suspicion be well founded (and the obscure or erroneous reasoning of its authors renders such a suspicion justifiable), there will be little difficulty in accounting for Sir Isaac's tardiness in publishing his discoveries. Under any other point of view his conduct appears inconsistent.

I shall most likely be accused of presumption in making the preceding remarks; but I would rather believe that the method of fluxions was the result of repeated trials, than that its author was indifferent about the progress of science, or that he wished to reserve to himself, as a miser does his gold, that which was a thousand times more valuable to his fellow men.

I am, sir, yours &c.

No. 2, Grove Terrace, March 14, 1821. THOMAS TREDGOLD.

XXXIII. *On*

XXXIII. *On the Action of the Voltaic Pile upon the Magnetic Needle.* *By M.* Boisgeraud *Jun.*

(*Read in the Academy of Sciences, 9th Nov.* 1820.)

I was witness in the sitting of the 11th September last, to some experiments relative to the action of the pile on the magnetic needle, which were repeated by M. Arago, and of which M. Œrsted is the inventor. These newly observed phænomena inspired me with a lively interest; I hastened to renew the experiments, and saw my attempt crowned with success. I did not stop at these first attempts; I made new trials, in which I obtained results which appeared to me interesting, and which may perhaps assist in explaining this sort of phænomena. I shall confine myself to reporting those which I conceive to be new.

I made use of a Galvanic trough of twenty pairs of the old construction, and of little power. The conducting wire was composed of two wires of platina of small diameter; they communicated with the poles of the pile, by means of brass wires of a diameter more considerable. I endeavoured to increase the effects of my first pile, by uniting it to a second exactly the same, in such a manner as to form a battery. This arrangement of the apparatus gave me occasion to ascertain that the metallic arc which joined the extreme poles, was not the only one susceptible of making the needle deviate; the intermediary arc, which united one of the piles to the other, produced the same effect, and observed the same laws. I was even able to approximate the effects of the two arcs; and according as they tended, by their respective positions, to produce a deviation in the same or in an opposite direction, the effects were augmented or diminished.

The *nature* of the conductors in these experiments appeared to me to have a great influence. Thus, a simple leaf of paper dipped in the liquid of the troughs of the pile sufficed to diminish the effect considerably, when it was interposed between the conducting wires of my apparatus; a short piece of charcoal, partly dipped in water, destroyed it entirely*.

The following, is in this respect, a fact remarkable enough. Having placed the magnetic needle under the *intermediary arc* in such a manner that a strong deviation was produced when I made the two extreme poles communicate by a metallic arc, it was not so when I established the communication with my two hands: however, I experienced a shock rendered the more dis-

* Some experiments since made by M. Ampere, with an apparatus of small power, proved that charcoal (especially if incandescent) does not entirely destroy the effects. Water charged with acid produces the same effect.

 agreeable

agreeable, from having a slight wound near that part of the finger which was plunged in one of the troughs of the pile.

I endeavoured, at the desire of M. Poisson, but in vain, to obtain with my apparatus effects at a distance, by terminating my wires of platina with very sharp points; contact was always necessary to produce a sensible deviation.

These facts have led me to conclude that the nullity of action in a Voltaic column of fifty-eight pairs (whether horizontal or vertical) must be attributed at least as much to the imperfect conducting power of the wrappers of wet cloth, as to the small dimension of the discs (that of about a six-franc piece). However, this pile gives a shock strong enough.

I remarked that it is very easy, by means of a battery composed of two piles, to establish the conducting power of liquid or gaseous substances interposed in one of the arcs, by placing the needle at the proximity of the other arc.

Hitherto I made use of a small compass needle suspended horizontally from a vertical pivot of brass. To render the phænomena of inclination more sensible, I took a steel wire of very small diameter, and magnetised it by a single touch*. It was about two centimetres in length, and manifested different degrees of magnetism, branching nearly from its centre point. I suspended this small needle by a very fine silk thread, in such a manner that it was horizontal.

By observing with attention all the circumstances which attended its deviations from the approach of the conducting wire of the pile, I perceived that every time the pole of the needle passed from one side to the other of this wire, though the latter was a little attracted on the passage of the needle, it did not prevent its movement; for the wire of suspension took an oscillatory movement, which approached and receded from the needle of the conducting wire; and when it had sufficiently receded, it took then a movement which could no longer affect that wire; but it avoided encountering the nearest face of the wire to take this new position, and it went, on the contrary, to the face of that side which it had passed.

The oscillations of the wire of suspension being modified by the action of a weight which serves to bring the needle to a point of equilibrium, from which another force would remove it; I thought of placing this small needle slightly coated by a greasy body; because then, the action of the weight being balanced, that of the conducting wire would have all its effect. The experiment succeeded perfectly.

I may observe in passing, that this small needle became sus-

* The effects are more marked, and more easily to be produced, with a wire magnetised to saturation by the method of double touch.

ceptible

ceptible of decomposing water on the approach of two wires branching from the poles of the pile. This phænomenon, which I believe was already known, is easily to be conceived, and is equally produced with needles of steel not magnetised, of silver, of platina, &c.

Notwithstanding the feeble magnetism of this small needle, when put out of the magnetic meridian it recovered it promptly enough. It presented the same phænomena of declination as the needle to the pivot, and that without sensible oscillations. But I have further observed, that when the conducting wire was horizontal, and in the magnetic meridian of the needle, the latter turned round its middle point, which remained fixed; which supposes a certain symmetry of effects, and appears to me to lead, besides, to the consequence, that, in that case, the existing forces decomposed, according to the plane of the surface of the water, are reduced to two equals, opposite to and equally removed from the middle point of the needle.

It remains to me now to describe the movements of this needle according to the magnetic meridian. I have formed a table of them. For the sake of brevity, I designated the direction of the conducting wire by always commencing at the extremity of that wire by which it approximates to the zinc pole of the pile. The indication of movements applies equally to the two poles of the needle, and they may be indefinitely prolonged on the surface of the water *. The expressions of *attraction* or *repulsion* indicate that the needle approximates or recedes from the conducting wire.

The conducting wire situated	Above the surface of the water and directed	From east to west †—repulsion. From west to east—attraction.
	Under the needle (in the water) and directed	From east to west—attraction. From west to east—repulsion.
	To the east of the needle and directed	Downwards—attraction. Upwards—repulsion.
	To the west of the needle and directed	Downwards—repulsion. Upwards—attraction.

When the conducting wire is quite perpendicular to the meridian plane, the movement of the needle has constantly taken place according to the magnetic meridian ‡, which requires that the forces then existing, decomposed according to the plane of the surface of the water, should give a resulting direction corresponding with the magnetic meridian. If moreover the mid-

* The water was distilled, in order to lessen the decomposing action of the pile. † These directions are always given according to the needle of the compass. ‡ In the contrary case the movement is involved with that of the declination.

dle

dle point of the needle is in the vertical plane passing by the conducting wire, it remains in a state of repose. This equilibrium is fixed, every time that in this position the conducting wire appears to exercise an attraction on the poles of the needle, and moveable, if it is directed in such a manner as to exercise a repulsion. This remarkable phænomenon seems to prove that the conducting wire acts only the poles of the needle.

The attractions and repulsions above mentioned, succeed in certain cases in a manner which merits observation. The needle of which I made use, was placed on the water in a glass stand, in such a manner that, to observe what passed when the conducting wire was under the needle, I plunged it in the water; and after having plunged it in the water, it came out to the right and left of the needle before touching the edges of the glass. I then drew some parallels to the meridian by the two points where this wire issued from the water. I divided the surface of the water into three parts; the one comprehended between the two wires, and which I called interior; the two others beyond them, or exterior. Thus placed as indicated in the preceding table, every time that there was attraction in the interior part, there was repulsion in the exterior parts, and *vice versâ*. For then the needle, after having been repulsed in one of the divisions of water, passed to that where it had attraction by a movement owing in part to a declination, and more or less slow according to the distance of the needle. Once drawn to the attractive side, it takes an accelerated movement, which continues till its middle comes opposite to the conducting wire. This middle point recedes slowly from the wire. I believe that the impossibility of touching it may be ascribed to a capillary effect.

It will be seen by these latter experiments, that there is no equilibrium for the needle subjected to the action of the conducting wire, except when its middle point is of all the points the nearest to the conductor.

I believe that, according as the needle, after being repulsed in one of the divisions of water, enters the other by the north or by the south, it has a greater tendency towards one part of the conducting wire than the other; but this law does not always hold. This tendency of certain faces of the needle, and of the conducting wire, to approximate, in preference to others which seem on the contrary to repel, appears to me to have great influence in these phænomena. It seems to me also, that the results of forces which produce these movements may be regarded as situated in a plane perpendicular to the direction of the conducting wire.

I attempted, lastly, a sort of experiment from which it is difficult to draw conclusions on account of their complexity. [The account of these experiments in our next.]

XXXIV. *No-*

XXXIV. *Notices respecting New Books.*

Narrative of the Operations and recent Discoveries within the Pyramids, Temples, Tombs, and Excavations in Egypt and Nubia; and of a Journey to the Coast of the Red Sea, in search of the ancient Berenice; and another to the Oasis of Jupiter Ammon. By G. Belzoni. 4to, with a Volume of Plates.

In our preceding volumes we took some notice of the discoveries of this enterprising traveller, who has done more in laying open the treasures of Egyptian antiquities than all who have gone before him. He has now laid before the public some account of his labours in a 4to volume of 500 pages, accompanied with a volume of plates, atlas folio. The work is from his own pen, but it needs no apology as to style; and as to matter, it is highly interesting, especially to the antiquarian. Our limits do not allow us to give large extracts from books of travels; but we shall give our readers some idea of the new matter which the author brings forward, by laying before them an account of what he himself considers as his principal discovery.

"On the 16th of October 1817, I recommenced my excavations in the valley of *Beban el Malock* [the Tombs of the Kings], and pointed out the fortunate spot which has paid me for all the trouble I took in my researches. I may call this a fortunate day, one of the best perhaps of my life; I do not mean to say that fortune has made me rich, for I do not consider all rich men fortunate; but she has given me that satisfaction, that extreme pleasure, which wealth cannot purchase; the pleasure of discovering what has been long sought in vain, and of presenting the world with a new and perfect monument of Egyptian antiquity, which can be recorded as superior to any other in point of grandeur, style, and preservation, appearing as if just finished on the day we entered it; and what I found in it will show its great superiority to all others. Not fifteen yards from the last tombs I described, I caused the earth to be opened at the foot of a steep hill, and under a torrent, which, when it rains, pours a great quantity of water over the very spot I have caused to be dug. No one could imagine, that the ancient Egyptians would make the entrance into such an immense and superb excavation just under a torrent of water; but I had strong reasons to suppose that there was a tomb in that place, from indications I had observed in my pursuit. The Fellahs who were accustomed to dig, were all of opinion that there was nothing in that spot, as the situation of the tomb differed from that of any other. I continued the work, however, and the next day, the 17th, in the evening,

evening, we perceived the part of the rock that was cut, and formed the entrance. On the 18th, early in the morning, the task was resumed, and about noon the workmen reached the entrance, which was eighteen feet below the surface of the ground. The appearance indicated that the tomb was of the first rate: but still I did not expect to find such a one as it really proved to be. The Fellahs advanced till they saw that it was probably a large tomb, when they protested they could go no further, the tomb was so much choked up with large stones, which they could not get out of the passage. I descended, examined the place, pointed out to them where they might dig, and in an hour there was room enough for me to enter through a passage that the earth had left under the ceiling of the first corridor, which is thirty-six feet two inches long, and eight feet eight inches wide, and, when cleared of the ruins, six feet nine inches high. I perceived immediately by the painting on the ceiling, and by the hieroglyphics in *basso relievo*, which were to be seen where the earth did not reach, that this was the entrance into a large and magnificent tomb. At the end of this corridor I came to a staircase twenty-three feet long, and of the same breadth as the corridor. The door at the bottom is twelve feet high. From the foot of the staircase I entered another corridor, thirty-seven feet three inches long, and of the same width and height as the other, each side sculptured with hieroglyphics in *basso relievo*, and painted. The ceiling is also finely painted, and in pretty good preservation. The more I saw, the more I was eager to see, such being the nature of man; but I was checked in my anxiety at this time, for at the end of this passage I reached a large pit, which intercepted my progress. This pit is thirty feet deep, and fourteen feet by twelve feet three inches wide. The upper part of the pit is adorned with figures, from the wall of the passage up to the ceiling. The passages from the entrance all the way to this pit have an inclination downward of an angle of eighteen degrees. On the opposite side of the pit facing the entrance, I perceived a small aperture two feet wide and two feet six inches high, and at the bottom of the wall a quantity of rubbish. A rope fastened to a piece of wood, that was laid across the passage against the projections which form a kind of door, appears to have been used by the ancients for descending into the pit; and from the small aperture on the opposite side hung another, which reached the bottom, no doubt for the purpose of ascending. We could clearly perceive, that the water which enters the passages from the torrents of rain ran into this pit, and the wood and rope fastened to it crumbled to dust on touching them. At the bottom of the pit were several pieces of wood, placed against the side of it, so as to assist the person who was

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to ascend by the rope into the aperture. I saw the impossibility of proceeding at the moment. Mr. Beechey, who that day came from Luxor, wished to enter the tomb, but was also disappointed.

"The next day, the 19th, by means of a long beam we succeeded in sending a man up into the aperture; and having contrived to make a bridge of two beams, we crossed the pit. The little aperture we found to be an opening forced through a wall, that had entirely closed the entrance, which was as large as the corridor. The Egyptians had closely shut it up, plastered the wall over, and painted it like the rest of the inside of the pit, so that, but for the aperture, it would have been impossible to suppose that there was any further proceeding; and any one would conclude that the tomb ended with the pit. The rope in the inside of the well did not fall to dust, but remained pretty strong, the water not having reached it at all; and the wood to which it was attached was in good preservation. It was owing to this method of keeping the damp out of the inner parts of the tomb, that they are so well preserved. I observed some cavities at the bottom of the well, but found nothing in them, nor any communication from the bottom to any other place; therefore we could not doubt their being made to receive the waters from the rain, which happens occasionally in this mountain. The valley is so much raised by the rubbish, which the water carries down from the upper parts, that the entrance into these tombs is become much lower than the torrents; in consequence, the water finds its way into the tombs, some of which are entirely choked up with earth.

"When we had passed through the little aperture, we found ourselves in a beautiful hall, twenty-seven feet six inches by twenty-five feet ten inches, in which were four pillars three feet square. I shall not give any description of the painting, till I have described the whole of the chambers. At the end of this room, which I call the entrance-hall, and opposite the aperture, is a large door, from which three steps lead down into a chamber with two pillars. This is twenty-eight feet two inches by twenty-five feet six inches. The pillars are three feet ten inches square. I gave it the name of the drawing-room; for it is covered with figures, which, though only outlined, are so fine and perfect, that you would think they had been drawn only the day before. Returning into the entrance-hall, we saw on the left of the aperture a large staircase, which descended into a corridor. It is thirteen feet four inches long, seven and a half wide, and has eighteen steps. At the bottom we entered a beautiful corridor, thirty-six feet six inches by six feet eleven inches. We perceived that the paintings became more perfect as we advanced further into the interior. They retained their gloss, or a

 kind

kind of varnish over the colours, which had a beautiful effect. The figures are painted on a white ground. At the end of this corridor we descended ten steps, which I call the small stairs, into another, seventeen feet two inches by ten feet five inches. From this we entered a small chamber, twenty feet four inches by thirteen feet eight inches, to which I gave the name of the Room of Beauties; for it is adorned with the most beautiful figures in *basso relievo*, like all the rest, and painted. When standing in the centre of this chamber, the traveller is surrounded by an assembly of Egyptian gods and goddesses. Proceeding further, we entered a large hall, twenty-seven feet nine inches by twenty-six feet ten inches. In this hall are two rows of square pillars, three on each side of the entrance, forming a line with the corridors. At each side of this hall is a small chamber: that on the right is ten feet five inches by eight feet eight inches; that on the left, ten feet five inches by eight feet nine inches and a half. This hall I termed the Hall of Pillars; the little room on the right, Isis' Room, as in it a large cow is painted, of which I shall give a description hereafter; that on the left, the Room of Mysteries, from the mysterious figures it exhibits. At the end of this hall we entered a large saloon, with an arched roof or ceiling, which is separated from the Hall of Pillars only by a step; so that the two may be reckoned one. The saloon is thirty-one feet ten inches by twenty-seven feet. On the right of the saloon is a small chamber without any thing in it, roughly cut, as if unfinished, and without painting: on the left we entered a chamber with two square pillars, twenty-five feet eight inches by twenty-two feet ten inches. This I called the Side-board Room, as it has a projection of three feet in form of a sideboard all round, which was perhaps intended to contain the articles necessary for the funeral ceremony. The pillars are three feet four inches square, and the whole as beautifully painted as the rest. At the end of the same room, and facing the Hall of Pillars, we entered by a large door into another chamber with four pillars, one of which is fallen down. This chamber is forty-three feet four inches by seventeen feet six inches; the pillars three feet seven inches square. It is covered with white plaster, where the rock did not cut smoothly, but there is no painting on it. I named it the Bull's, or Apis' Room, as we found the carcase of a bull in it, embalmed with asphaltum; and also, scattered in various places, an immense quantity of small wooden figures of mummies six or eight inches long, and covered with asphaltum to preserve them. There were some other figures of fine earth baked, coloured blue, and strongly varnished. On each side of the two little rooms were some wooden statues standing erect, four feet high, with a circular hollow inside, as if

to

to contain a roll of papyrus, which I have no doubt they did. We found likewise fragments of other statues of wood and of composition.

"But the description of what we found in the centre of the saloon, and which I have reserved till this place, merits the most particular attention, not having its equal in the world, and being such as we had no idea could exist. It is a sarcophagus of the finest oriental alabaster, nine feet five inches long, and three feet seven inches wide. Its thickness is only two inches; and it is transparent when a light is placed in the inside of it. It is minutely sculptured within and without with several hundred figures, which do not exceed two inches in height, and represent, as I suppose, the whole of the funeral procession and ceremonies relating to the deceased, united with several emblems, &c. I cannot give an adequate idea of this beautiful and invaluable piece of antiquity, and can only say, that nothing has been brought into Europe from Egypt that can be compared with it. The cover was not there: it had been taken out, and broken into several pieces, which we found in digging before the first entrance. The sarcophagus was over a staircase in the centre of the saloon, which communicated with a subterraneous passage, leading downwards, three hundred feet in length. At the end of this passage we found a great quantity of bats' dung, which choked it up, so that we could go no further without digging. It was nearly filled up too by the falling in of the upper part. One hundred feet from the entrance is a staircase in good preservation; but the rock below changes its substance, from a beautiful solid calcareous stone, becoming a kind of black rotten slate, which crumbles into dust only by touching. This subterraneous passage proceeds in a south-west direction through the mountain. I measured the distance from the entrance, and also the rocks above, and found, that the passage reaches nearly half way through the mountain to the upper part of the valley. I have reason to suppose, that this passage was used to come into the tomb by another entrance; but this could not be after the death of the person who was buried there, for at the bottom of the stairs just under the sarcophagus a wall was built, which entirely closed the communication between the tomb and the subterraneous passage. Some large blocks of stone were placed under the sarcophagus horizontally, level with the pavement of the saloon, that no one might perceive any stairs or subterranean passage was there. The door-way of the sideboard room had been walled up, and forced open, as we found the stones with which it was shut, and the mortar in the jambs. The staircase of the entrance-hall had been walled up also at the bottom, the space filled with rubbish, and the floor covered with large

 blocks

blocks of stone, so as to deceive any one who should force the fallen wall near the pit, and make him suppose that the tomb ended with the entrance-hall and the drawing-room. I am inclined to believe, that whoever forced all these passages must have had some spies with them, who were well acquainted with the tomb throughout. The tomb faces the north-east, and the direction of the whole runs straight south-west.

"To give an accurate description of the various representations within this tomb, would be a work above my capacity. I shall therefore only endeavour to describe the most remarkable that are to be seen in the various parts of it. From these the reader may form some idea of this magnificent excavation.

"The entrance into the tomb is at the foot of a high hill, with a pretty steep ascent. The first thing the traveller comes to is a staircase cut out of the rock, which descends to the tomb. The entrance is by a door of the same height as the first passage. I beg my kind reader to observe, that all the figures and hieroglyphics of every description are sculptured in *basso relievo*, and painted over, except in the outlined chamber, which was only prepared for the sculptor. This room gives the best ideas that have yet been discovered of the original process of Egyptian sculpture. The wall was previously made as smooth as possible; and where there were flaws in the rocks, the vacuum was filled up with cement, which, when hard, was cut along with the rest of the rock. Where a figure or any thing else was required to be formed, after the wall was prepared, the sculptor appears to have made his first sketches of what was intended to be cut out. When the sketches were finished in red lines by the first artist, another more skilful corrected the errors, if any, and his lines were made in black, to be distinguished from those which were imperfect. When the figures were thus prepared, the sculptor proceeded to cut out the stone all round the figure, which remained in *basso relievo*, some to the height of half an inch, and some much less, according to the size of the figure. For instance: if a figure were as large as life, its elevation was generally half an inch; if the figure were not more than six inches in length, its projection would not exceed the thickness of a dollar, or perhaps less. The angles of the figures were all smoothly rounded, which makes them appear less prominent than they really are. The parts of the stone that were to be taken off all round the figure did not extend much further, as the wall is thickly covered with figures and hieroglyphics, and I believe there is not a space on those walls more than a foot square without some figure or hieroglyphic. The garments, and various parts of the limbs, were marked by a narrow line, not deeper than the thickness of a half-crown, but so exact that it produced the intended effect.

"When the figures were completed and made smooth by the sculptor, they received a coat of whitewash all over. This white is so beautiful and clear, that our best and whitest paper appeared yellowish when compared with it. The painter came next, and finished the figure. It would seem as if they were unacquainted with any colour to imitate the naked parts, since red is adopted as a standing colour for all that meant flesh. There are some exceptions indeed; for in certain instances, when they intended to represent a fair lady, by way of distinguishing her complexion from that of the men, they put on a yellow colour to represent her flesh. Yet it cannot be supposed that they did not know how to reduce their red paints to a flesh colour; for on some occasions, where the red flesh is supposed to be seen through a thin veil, the tints are nearly of the natural colour; if we suppose the Egyptians to have been of the same hue as their successors, the present Copts, some of whom are nearly as fair as the Europeans. Their garments were generally white, and their ornaments formed the most difficult part, when the artists had to employ red in the distribution of the four colours, in which they were very successful. When the figures were finished, they appear to have laid on a coat of varnish; though it may be questioned, whether the varnish were thus applied, or incorporated with the colour. The fact is, that nowhere else except in this tomb is the varnish to be observed, as no place in Egypt can boast of such preservation, nor can the true customs of the Egyptians be seen any where else with greater accuracy.

"With the assistance of Mr. Ricci, I have made drawings of all the figures, hieroglyphics, emblems, ornaments, &c. that are to be seen in this tomb; and by great perseverance I have taken impressions of every thing in wax: to accomplish the work has been a laborious task, that occupied me more than twelve months.

"The drawings show the respective places of the figures, so that if a building were erected exactly on the same plan, and of the same size, the figures might be placed in their situations precisely as in the original, and thus produce in Europe a tomb in every point equal to that in Thebes, which I hope to execute if possible.

"Immediately within the entrance into the first passage, on the left hand, are two figures as large as life, one of which appears to be the hero entering into the tomb. He is received by a deity with a hawk's head, on which are the globe and serpent. Both figures are surrounded by hieroglyphics; and further on, near the ground, is a crocodile very neatly sculptured. The walls on both sides of this passage are covered with hieroglyphics, which are separated by lines from the top to the bottom, at the distance

distance of five or six inches from one another. Within these lines the hieroglyphics form their sentences; and it is plainly to be seen, that the Egyptians read from the top to the bottom, and then recommenced at the top. The ceiling of this first passage is painted with the figure of the eagles (as shown in Plate 2). Beyond the first passage is a staircase with a niche on each side, adorned with curious figures with human bodies and the heads of various animals, &c. At each side of the door at the bottom of the stairs is a female figure kneeling, with her hands over a globe. Above each of these figures is the fox, which, according to the Egyptian custom, is always placed to watch the doors of sepulchres. On the front space over the door are the names of the hero and his son, or his father, at each side of which is a figure with its wings spread over the names to protect them (as shown in Plate 3). The names are distinguished by being inclosed in two oval niches. In that of Nichao is a sitting figure, known to be a male by the beard. He has on his head the usual corn measure, and the two feathers; on his knees the sickle and the flail: over his head is a crescent with the horns upward: above which is what is presumed to be a faggot of various pieces of wood bound together, and by its side a group twisted in a serpentine form. Behind the figure are what are thought by some to be two knives, by others feathers; but as the feathers are of a different form, I for my part think they are sacrificing knives, which may have served as emblems of the priesthood, for we know that the heroes or kings of Egypt were initiated into the sacred rites of the gods. Below the figure is a frame of two lines drawn parallel to each other, and connected by similar lines, beneath which is the emblem of moving water.

"In the next oval on the right is a sitting female figure with a band round the head fastening a feather, and on her knees she holds the keys of the Nile. Above the head is the globe, and beneath the figure the form of a tower, as it is supposed to represent strength. The faces of both figures are painted blue, which is their colour of the face of the great God of the creation. On each of the oval frames there is the globe and feathers, and beneath it two hieroglyphics not unlike two overflowing basins, as they are under the two protecting figures at each side of the oval frame.

"Next is the second passage, on the right hand side of which are some funeral processions, apparently in the action of taking the sarcophagus down into the tomb, the usual boat, which carries the male and female figures upon it, and in the centre the boat with the head of the ram drawn by a party of men.

"The wall on the left is likewise covered with similar processions. Among them is the *scarabæus*, or beetle, elevated in the

the air, and supported by two hawks, which hold the cords drawn by various figures; and many other emblems and symbolical devices. The figures on the wall of the well are nearly as large as life. They appear to represent several deities; some receiving offerings from people of various classes.

"Next is the first hall, which has four pillars in the centre, at each side of which are two figures, generally a male and a female deity. On the right hand side wall there are three tiers of figures one above the other, which is the general system almost all over the tomb. In the upper tier are a number of men pulling a chain attached to a standing mummy, which is apparently unmoved by their efforts. The two beneath consist of funeral processions, and a row of mummies lying on frames horizontally on the ground. On the left is a military and mysterious procession, consisting of a great number of figures all looking toward a man who is much superior to them in size, and faces them. At the end of this procession are three different sorts of people, from other nations, evidently Jews, Ethiopians, and Persians. Behind them are some Egyptians without their ornaments, as if they were captives rescued and returning to their country, followed by a hawk-headed figure, I suppose their protecting deity. (This procession is represented on Plates 6, 7, and 8.)

"I have the satisfaction of announcing to the reader, that, according to Dr. Young's late discovery of a great number of hieroglyphics, he found the names of Nichao and Psammethis his son, inserted in the drawings I have taken of this tomb. It is the first time that hieroglyphics have been explained with such accuracy, which proves the doctor's system beyond doubt to be the right key for reading this unknown language; and it is to be hoped that he will succeed in completing his arduous and difficult undertaking, as it would give to the world the history of one of the most primitive nations, of which we are now totally ignorant. Nichao conquered Jerusalem and Babylon, and his son Psammethis made war against the Ethiopians. What can be more clear than the above procession? The people of the three nations are distinctly seen. The Persians, the Jews, and the Ethiopians, come in, followed by some captive Egyptians, as if returning into their country, guarded by a protecting deity. The reason why the Egyptians must be presumed to have been captives is, their being divested of all the ornaments which served to decorate and distinguish them from one another. The Jews are clearly distinguished by their physiognomy and complexion, the Ethiopians by their colour and ornaments, and the Persians by their well-known dress, as they are so often seen in the battles with the Egyptians.

"In the front of this hall, facing the entrance, is one of the finest

finest compositions that ever was made by the Egyptians, for nothing like it can be seen in any part of Egypt. It consists of four figures as large as life. The god Osiris sitting on his throne receiving the homages of a hero, who is introduced by a hawk-headed deity. Behind the throne is a female figure as if in attendance on the great god. The whole group is surrounded by hieroglyphics, and inclosed in a frame richly adorned with symbolical figures. The winged globe is above, with the wings spread over all, and a line of serpents crowns the whole. The figures and paintings are in such perfect preservation, that they give the most correct idea of their ornaments and decorations. (Plate 19 is copied from this composition.)

"Straight forward is the entrance into another chamber with two pillars. The wall of this place is outlined, ready for the sculptor to cut out his figure. It is here that we may plainly see the manner in which the artist prepared the figure on the wall ready to be cut; and it is almost impossible to give a description of the various figures which adorn the walls and pillars of this chamber. There are great varieties of symbolical figures of men, women, and animals, apparently intending to represent the different exploits of the hero to whom the tomb was dedicated.

"On going out of this chamber into the first hall is a staircase, which leads into a lower passage, the entrance into which is decorated with two figures, one on each side, a male and a female, as large as life. The female appears to represent Isis, having, as usual, the horns and globe on her head. She seems ready to receive the hero, who is about to enter the regions of immortality. The garments of this figure are so well preserved, that nothing which has yet been brought before the public can give a more correct idea of Egyptian costume. The figure of the hero is covered with a veil, or transparent linen, folded over his shoulder, and covering his whole body, which gives him a very graceful appearance. Isis is apparently covered with a net, every mesh of which contains some hieroglyphic, serving to embellish the dress of the goddess. The necklace, bracelets, belt, and other ornaments, are so well arranged, that they produce the most pleasing effect, particularly by the artificial lights, all being intended to conduce to this purpose (shown in Plate 18).

"On the wall to the left, on entering this passage, is a sitting figure of the size of life: it is the hero himself on his throne, having the sceptre in his right hand, while the left is stretched over an altar, on which are twenty divisions (shown in Plate 1). A plate in the form of an Egyptian temple is hung to his neck by a string. It contains an obelisk and two deities—one on each side of it. Plates of this kind have been much sought after, as they appear to have been

been the decoration or breastplate of the kings of Egypt. Few have been found, and I have seen only two,—one is in the British Museum, and the other I was fortunate enough to procure from an Arab, who discovered it in one of the tombs of the kings in Beban el Malook. It is of black basalt, much larger and superior in workmanship to the other, which proves that they were of various sizes, and more or less finished. It has the *scarabæus* or beetle in *alto relievo* on a small boat with a deity on each side of it, and on the reverse is the usual inscription. Over the head of this figure is the eagle with extended wings, as if protecting the king. On the upper part of each side of the walls of the passage is the history of the hero divided into several small compartments nearly two feet square, containing groups of figures eighteen inches high. The hero is to be seen every where standing on a heap of corn, receiving offerings from his soldiers or companions in war. Further on is a small staircase leading into a short passage, where the procession still continues, and the sacrifice of a bull is to be seen (shown in Plate 13). The walls of both passages are covered with hieroglyphics in separate divisions. From this short passage there is an entrance into another much wider than the rest. The charming sight of this place made us give it the name of the Room of Beauties. All the figures are in such perfection, that the smallest part of their ornaments can be clearly distinguished. The sides of the doors are most beautifully adorned with female deities, surrounded with hieroglyphics, and the *lotus* is to be seen both in bud and in full bloom, with the serpent on a half globe over it (Plate 17). Further on is the great hall with six pillars, containing on each side of it, two figures as large as life. The walls are adorned with the procession and other symbolical figures. Over the door, in the inside, is the figure of a female with extended wings. At each side of this hall is a small cell; that on the left containing various mummies and other figures, and that on the right a cow of half the natural size, with a number of figures under it, which form a very curious group. The walls also are covered with hieroglyphics. In the large hall close to the door are a number of men carrying a long slender pole, at each end of which is a cow's head, and on the pole, two bulls (shown in Pl. 15). Still further, the hall opens into the large vaulted chamber. It would be impossible to give any description of the numerous figures which adorn the wall of this place. It was here that the body of the king was deposited, as I found in its centre the beautiful sarcophagus. This is sculptured within and without with small figures in intaglio, coloured with a dark blue, and, when a light is put into the inside of it, it is quite transparent. The ceiling of the vault itself is painted blue, with a procession of figures and other groups relating to the zodiac.

"The next is a chamber with a projection like a sideboard. It

has

has two square pillars with two figures on every side (Plates 4 and 5). The walls in every part of this chamber are also beautifully adorned with symbolical figures. It is useless to proceed any further in the description of this heavenly place, as I can assure the reader he can form but a very faint idea of it from the trifling account my pen is able to give. Should I be so fortunate, however, as to succeed in erecting an exact model of this tomb in Europe, the beholder will acknowledge the impossibility of doing it justice in a description."

In closing our extract from this interesting work, we cannot avoid noticing a rather curious fact, that 15 walking figures in Plates 6, 7, and 8, and two standing figures in Plate 17, have each of them two left hands—that is, a left hand hung from their right shoulder; and one figure in Plate 13 has two right hands. We wish Mr. Belzoni to state, whether they are so in the originals, or whether this be an error resulting from any mechanical means employed in multiplying similar figures when making the drawings.

A Geological Classification of Rocks, with descriptive Synopses of the Species and Varieties; comprising the Elements of practical Geology. By JOHN MACCULLOCH, M.D. F.R.S. F.L.S. Vice Pres. Geol. Soc. &c.

A work under this title has been lately published, and we take the first opportunity of announcing it to our geological readers. It fills up a blank among the elementary writings which have been published for the use of students in geology, and supplies a want which must have been felt by every one engaged in that pursuit.

We have not room to give an analysis of a work extending to near 700 pages; nor is it indeed, from its very nature, susceptible of much abridgement. A very bare sketch of the plan must suffice, as we presume that there are few cultivators of this science who will not have recourse to the work itself.

The basis of arrangement is geological; or the rocks are disposed in families under their ancient titles (unless where certain alterations appeared necessary) on a plan analogous to that of the celebrated Werner. We have copied the table of these families, as it will serve to give our readers a general idea of the arrangement.

PRIMARY CLASS.

Unstratified.

Granite.

Stratified.

Gneiss.	Red sandstone.
Micaceous schist.	Argillaceous schist.
Chlorite schist.	Diallage rock.
Talcose schist.	Limestone.
Hornblende schist.	Serpentine.
Actinolite schist.	Compact felspar.
Quartz rock.	

SE-

SECONDARY CLASS.

Stratified.

Lowest (red) sandstone.	Limestone.
Superior sandstone.	Shale.

Unstratified.

Overlying and venous rock.	Pitchstone.

OCCASIONAL ROCKS.

Jasper.	Gypsum.
Siliceous schist.	Conglomerate rocks
Chert.	Veinstones.

APPENDIX.

Volcanic rocks.	Alluvia.
Clay, marle, sand.	Lignite.
Coal.	Peat.

The known abilities of the author, his zeal in this branch of science, and his careful discrimination in examining the character of every natural product that comes under his observation, are too well known to geologists to render it necessary for us to say any thing in commendation of the work. We shall only say, in one word, that it will not disappoint those practical geologists who apply to it for information or assistance.

To each of these families is prefixed a geological account of its connexions, and of its position in the order of nature, together with a sketch of its prevailing mineral characters and composition. This is followed by a detailed synopsis of every variety which has come under the author's notice; all of them being minutely described in such a manner as to enable a student to ascertain any specimen by a reference to the table.

In a number of preliminary chapters there are given many matters essential to the general history and knowledge of rocks; and, among others, there are some appropriated to the purpose of enabling the student to discover the place and name of a rock from the nature of its component minerals. There is also a detailed account of the reasons for adopting a geological in preference to a mineralogical classification, which appear to us satisfactory. We shall take an opportunity in a future number of extracting some article for the purpose of giving our readers a specimen of the design and execution of this work.

Lately published.

Remarks on a Communication published in the xxth Number of the Journal of Literature, Science and the Arts, entitled "Observations on the Chemical Part of the Evidence given on the late Trial of the Action brought by Messrs. Severn, King and Co.

 against

against the Imperial Insurance Company, by Sam[l] Parkes, F.L.S. M.R.I.A. M.G.S. &c." By Richard Phillips, F.R.S. E. &c.; Philip Taylor; J. G. Children, F.R.S. &c.; John Martineau, jun.; John Bostock, M.D. F.R.S. &c.; and John Taylor, M.G.S. 8vo. 96 pages. 1*s*. 6*d*.

This pamphlet, from the circumstance of its consisting of distinct papers by the different gentlemen named in the title page, and all on the same subject, presents several repetitions which might have been avoided by condensing the whole into one article. The writers complain, and we think justly, of a want of candour and a suppression of facts in Mr. Parkes's communication; and they support these charges by quotations from the printed Report of the Trial, the effect of which it will be impossible for Mr. Parkes to set aside.

A Grammar of Botany, illustrative of artificial as well as natural Classification; with an Explanation of Jussieu's System. By Sir James Edward Smith, M.D. F.R.S. &c. President of the Linnæan Society. 8vo. 21 Plates. 12*s*. plain. 1*l*. 11*s*. 6*d*. coloured.

History of Northumberland, in Three Parts. By the Rev. John Hodgson. Vol. 5, being the 1st vol. of Part III. 4to. 2*l*. 2*s*. Royal paper 3*l*. 3*s*.

History of the several Italian Schools of Painting, with Observations. By J. T. James, M.A. 8vo. 9*s*. 6*d*.

An Engraved Series of Picturesque Views in Paris and its Environs, consisting of Views on the Seine, Public Buildings, Characteristic Scenery, &c. from original Drawings. By French Nash, Esq.

A Decimal Interest Table, constructed on new Principles. By Fred. Miller. 5*s*.

An Essay on Sea Bathing. By J. W. Williams, Surgeon. 12mo. 6*s*. 6*d*.

Practical Observations in Midwifery. By John Ramsbottom, M.D. 8vo. Part I. 10*s*. 6*d*.

Illustrations of the great Operations of Surgery. By Charles Bell. Part I. Plates coloured, 1*l*. 1*s*.

Communications on some of the most important Diseases of Children. By J. Clarke, M.D. Royal 8vo. 10*s*. 6*d*.

Dr. Cooke's History and Method of Cure of the various Species of Palsy. 8vo. 6*s*. 6*d*.

A Monthly Journal of Popular Medicine, explaining the Nature, Causes, and Prevention of Disease, the immediate Management of Accidents, and the Means of preserving Health. Conducted by Charles Thomas Haden, Surgeon to the Chelsea and Brompton Dispensary, &c. No. 1, for March 1821. 1*s*. 6*d*.

Rome in the Nineteenth Century, containing a complete Account

count of the Ruins of the ancient City, the Remains of the Middle Ages, and the Monuments of modern Times. 3 Vols. Post 8vo. 1*l.* 7*s.*

A Treatise on the Art of Weaving, illustrated with Plates. By John Murray. No. I. pp. 40, 8vo. 1*s.*—The whole of this work will be comprised in 12 or 13 numbers; and being quite of a practical nature, and by a person who seems well acquainted with his subject, we think it cannot but prove highly useful to those who are practically or commercially engaged in matters connected with the fabrication of woven articles.

Part the First of a General Catalogue of Ancient and Modern Books, for the year 1821-2. By Lackington, Hughes, Harding, Mavor and Lepard, Finsbury-square.

Memoirs of the Revolution of Mexico, with a Narrative of the Campaign of General Mina, Anecdotes of his Life, and *Observations on the Practicability of connecting the Pacific and Atlantic Oceans by means of navigable Canals.* By W. D. Robinson, esq. 2 Vols. 8vo.

Remarks on a Bill now before Parliament, to amend the General Laws for regulating Turnpike Roads. By Benjamin Wingrave, General Surveyor of the Bath Turnpike Roads. 8vo. 1*s.* 6*d.*

This pamphlet presents various considerations well deserving of attention.

The Labourer's Friend and Handicraft's Chronicle, Nos. 1 & 2. To be continued monthly, at 6*d.*—The price and pretensions of this publication are both so modest, while it manifests a considerable portion of industry, and a strong desire to give useful information to the class for which it is intended, that we cannot avoid noticing, and at the same time recommending it as a useful publication.

Preparing for Publication.

Forty correct Views from Nature, of remarkable Places, taken on a Tour chiefly in Italy, by Cumberland sen., in two Numbers, each one guinea. A letter-press Description of each Scene will appear in the second Number.

Dr. Leach has nearly completed his Synopsis of British Mollusca.

A Translation of Otto Von Kotzebue's Narrative of a Voyage round the World, in the Russian ship Revric.

The Linnæan Genera of Insects; illustrated by 86 coloured Plates and general Observations on each Genus, by Mr. Wood, is now in the press.

Elementary Illustrations of the Celestial Mechanics of Laplace, comprehending the First Book for Students in the Mathematics. 8vo.

An Account of the interior of Ceylon, and its Inhabitants, with Travels in that Island. By John Davy, M.D. F.R.S.

The Study of Medicine, comprising its Physiology, Pathology and Practice. 3 Vols. 8vo. By Dr. Good.

Travels in Northern Africa from Tripoli to Mourzouk, the capital of Fezan, and thence to the southern extremity of that kingdom, in 1818, 19 and 20. By George Francis Lyon, Lieut. of the Royal Navy, and Companion of the late Mr. Ritchie.

A Treatise of Naval Gunnery. By Col. Sir Howard Douglas, Bart. K.S.C. C.B. F.R.S. &c. 8vo. Plates.

A Practical Treatise on the Hydrocephalus. By Leopold Anthony Golis, Physician, Vienna.

The Elements of Natural History translated from the German of Blumenbach, with considerable additions. By Clarke Abel, M.D. F.R.S. and F.L.S.

XXXV. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

March 9. The paper of Mr. Troughton, on the Construction and Use of the Repeating Circle and Altitude and Azimuth Circle, was concluded. In this paper the author describes the merits and demerits of each instrument, in the various purposes to which they may be applied; and closes his remarks by giving his decided preference to the Altitude and Azimuth Circle.—The next Meeting of the Society will be on April 13th.

PHRENOLOGICAL SOCIETY, EDINBURGH.

Many of our readers may not even have heard of the existence of this society, which was formed about two years ago for the purpose of examining the doctrines propagated by Drs. Gall and Spurzheim; and much less, perhaps, would they have expected that at Edinburgh, of all places, after the doctrines of these craniologists had been doomed to eternal oblivion in the Edinburgh Review, gentlemen should have been found ready to give the subject that investigation and free discussion which is necessary to elicit truth in all cases of doubt. We have not the honour of any personal acquaintance with the founders of this new system, but we have often heard others speak of Dr. Spurzheim's wonderful powers and happy talent for observation. By what process of reasoning he was led to conclude, or by what spirit of prophecy he was actuated, when he pitched upon Edinburgh as the destined spot from which the doctrines of phrenology were, at some future day, to be propagated, we are not informed; but we have been favoured with an anonymous communication on this subject from

from one who, we should conceive, from the displeasure he expresses at the Edinburgh Review, must be a decided disciple of the doctrines, if not a member of the Phrenological Society. We shall here give an extract from the communication.

According to our correspondent, "the good people of Edinburgh say that the air of intelligence, the spirit of inquiry, and the universal diffusion of University education was the cause of the Doctor's favourable prognosis:" but whether the Doctor actually reasoned thus or not, our anonymous friend seems willing to ascribe the fair play which the Doctor has ultimately met with in that quarter to another cause, no less powerful in its consequences wherever a sufficient degree of intelligence may be found under similar circumstances. He considers the inhabitants of Edinburgh as being, "for a considerable portion of the year, in the circumstances of the Athenians of old, *who spent their time in nothing else but either to tell or hear some new thing*."—But we shall suffer our correspondent to tell his own story:—

"I am inclined to ascribe the free inquiry into the doctrines of phrenology in Edinburgh, chiefly to the state of society in that city. Whoever has lived in the country, or in a small town, and more especially if it have no commercial connexions to keep the minds of the inhabitants on the tenter-hooks of anxiety, must have observed the rapidity with which all reports that once get into the vortex of circulation are disseminated. Now Edinburgh is precisely such a gossiping circle. Although its population be reckoned at about 100,000, it has no commerce, and for one half of the year the courts of justice do not sit, during which time two thirds of the population are absolutely idle; which recession from business gives them full leisure to inquire into the affairs of their neighbours, as well as into those of the world at large. Miss Jenny cannot get married quietly, the whole town knows every circumstance connected with the affair from the day the proposals were made to that which is to crown her wishes: a stranger cannot set his foot, or his nose, within the precincts of the place, but the whole population gets into full chase to hunt him out: the father must be informed of his family; the mother, if he be young, will take his fortune, profession, married or single state, under her own more immediate inspection—but of course without any eye to her own blooming daughter, advancing, in spite of fate, out of her teens, in which she has stuck fast for the last fifteen years. Should the stranger be aged and appear rich, every engine is set in motion to discover his connexions: and if no cousin fewer than twenty degrees removed be hit upon, then will all the arts of attentiveness and insinuation be put in requisition, and the unfortunate man is not unfrequently worried with kindness. To this peculiar state of society then, to the

the anxious pursuit of novelty, and to the still more anxious curiosity, an itching to know what the rest of their fellow citizens are doing, may, in a great measure, be attributed the extensive canvass of the doctrines of phrenology, and their consequent rapid and wide-spread diffusion in the capital of Scotland.

"When it is recollected that the Edinburgh press gave birth to that production, which, had it been in the power of invective and abuse—of assertions without proof—would have buried alike the founders and their system in infamy and oblivion,—it must excite surprise to see the hydra rearing its head from the hot-bed of opposition—from the very place where the whole system was proclaimed to the world as a deliberate fabrication, and Dr. Spurzheim himself held up as a liar, a quack, an impostor, and a vagabond. Whoever it was that wrote that paragon of illiberality which graced the Edinburgh Review some years ago, it is not necessary we should know; but whilst the work shall be read, it must remain an indelible stain on the candour and justice of that publication, which for so many years has swayed the literary sceptre; and, when it shall be viewed in its true colours, will show the public the broken reed on which they have so long depended for a true and faithful account of the merits and demerits of the different works brought under consideration. Against all the laws of hospitality, against all the rules of equity, we have traduced, most wantonly and unjustly, a foreigner but partially acquainted with our language, and still less with our manners and customs; who in a great measure threw himself on our indulgence, and only appealed to our justice! Alone and unfriended he stood on the conscious rectitude of his intentions, asked but a patient hearing, a candid inquiry: and we refused to grant so small a boon.

"Can this be that country so long celebrated for the hospitality of its people, for its impartial administration of justice? or has the veil which blinded the rest of the world been suddenly torn off? has the mask of hypocrisy at last fallen? has the wolf in sheep's clothing been no longer able to disguise the natural ferocity of his disposition? Be this as it may, all men are not callous to the claims of justice: some few individuals were found to extend their countenance to the stranger, and, if they did not blindly assent to his doctrines, at least did not condemn them unheard. Many, from the generous motive which first tempted them to extend their hand to an abused and learned individual, have found their acquaintance ripen into a friendship cemented by the strongest esteem and respect, and have never ceased to regard the moment that brought them into contact, as one of the most happy of their lives. To remove every unfavourable impression which the article we allude to may have left on the minds of any in regard to the personal character of Dr. Spurzheim, some pains have been

been taken to probe to the bottom all the circumstances attending his visit to this country; and it is but justice to say, that amongst the great number of persons who have been applied to on the subject, and who were personally acquainted with him, not a single individual has been found who does not speak in terms of the highest respect of his character as a gentleman, of the easy elegance of his manners, of the unassuming simplicity of his whole deportment, of the suavity and amenity of his disposition. As to his being a fortune-hunter, a character always looked upon with distaste in this country, till the accuser shall offer evidence of the fact the charge will be held as neither more nor less than a deliberate falsehood, raised by his enemies for the purpose of depreciating him in the eyes of the public. On the contrary, from every inquiry, it appears that money was one of the articles about which he was perhaps too little solicitous; and we may relate an anecdote of one of our friends, who, when the Doctor came to England first, called for him under the most unfavourable impressions, and, on requesting a decipherment of his own head, begged leave, as he conceived usual on such occasions, to present him with a fee. Instead of the German Doctor pocketing the cash, as my friend had anticipated, to his surprise the money was pushed back by Dr. Spurzheim, while a flush of generous indignation, at seeing his professions and his character so much misrepresented, crossed his cheek; and further, on my friend finding he had been misinformed, and saying he would attend the lectures then about to commence, and again tendering the money, 'No, no,' said the Doctor, 'come and hear first: if you like the subject then, and are anxious to learn, I shall teach you.' This extraordinary interview was the commencement of an acquaintance which time ripened into friendship, which is now so strongly tied by the bands of mutual affection and esteem, that nothing on this side the grave can divide them. In Edinburgh, too, I have heard Dr. Barclay publicly declare, that, so far as he had been able to discover, Dr. Spurzheim did not care for money; he had declined Dr. B.'s offer of a loan, having no occasion for it; and he gave free admission to his lectures to above 200 students who attended Dr. B.'s anatomical course.

"Dr. Spurzheim was in Wales when the wanton attack on him in the Edinburgh Review was published, and bent on a very different route; but his almost direct course was to Scotland, there openly to meet the charges and calumnies of his detractor. Does any thing in all this savour of the quack or the fortune-hunter? Who has ever found that a quack doctor was known to refuse a fee? Such a prodigy must have found its way into some record or another, but they are all equally silent. In one word, the whole charge is monstrous, absurd, and totally without

foundation. Had the reviewer of the physiognomical system kept in his recollection a declaration made in a previous number, he would not so hastily, unwarrantably, and unfairly have traduced Dr. Spurzheim. The following is the passage alluded to:

"'The most effectual way of persuading the public that their opinions are refuted out of a regard to human happiness, is to treat their authors with some small degree of liberality and gentleness. It is also pretty generally taken for granted, that a very angry disputant is usually in the wrong; that it is not a sign of much confidence in the argument, to take advantage of the unpopularity or legal danger of the opposite doctrine; and that when an unsuccessful and unfair attempt is made to discredit the general ability or personal worth of an antagonist, no great reliance is understood to be placed on the argument by which he may be lawfully opposed.'"—(Edinburgh Rev., No. 26. page 352.)

Our correspondent, after some other remarks, proposes that, "for the satisfaction of many, it is highly desirable that an association should be formed in London on a plan somewhat similar to that of Edinburgh; which, by the number, the accuracy, and the extent of its observations, shall soberly decide, whether or no the many respectable individuals who have espoused the doctrines of Spurzheim have been the dupes of designing and artful persons? Whether those who have so long opposed it shall be proved wrong? and, as a necessary consequence of the whole, Whether the science of phrenology is to sink entirely, or be fully established as a branch of general science? Those who may approve of this suggestion are requested to make their wish known by letter addressed to A. Z., to be left at the Star Office, Picket Place; and should a small additional number of individuals be found willing to associate themselves with a few who are already collected, they will receive notice of any future arrangement for connecting themselves with the Friends of the Study of Man."

London, March 23, 1821. A. Z.

MADRAS LITERARY SOCIETY.

This society met at the College Hall on the evening of the 1st Sept. last; when the Hon. Sir John Newbolt presided for the last time, previous to his embarkation for Europe.

The meeting was very numerously attended, and throughout its proceedings evinced the high respect and regard entertained by all present for their learned and worthy president, whose approaching departure from India could not but be felt as a great loss to the institution, and a source of much regret to every individual connected with it.

The following communications were read or laid before the society at this and the previous meeting:

Two

Two valuable papers from C. W. Whish, Esq. at Calicut:

1. On the alphabetical notation of the Hinduhs.

2. On the Hinduh quadrature of the circle, and the infinite series for the proportion of the circumference to a diameter, exhibited in the four Shasters, Tantra, Sangraham, Yukti, Chasha, Karanu-Padhatih, and Sadratna Matan. As also, by the same gentleman, copies of two inscriptions on stone in the Tiruvanoor pagoda, near Calicut; with copies in modern Tamul letters, and a translation of one of them into English.

A letter from J. Munro, Esq. at Tellicherry, accompanied by a donation of a curious old Persian and Latin work by Lewis De Dieu.

A letter from the secretary of the Asiatic Society, presenting seven volumes of their Researches to the society.

A considerable number of drawings of the various tribes and castes, male and female, composing the community of Malayalam, were laid on the table. These are understood to give a very accurate idea of the characteristic features of those people, and were executed by an able native artist, under the immediate direction of a gentleman, to whose kindness and zeal in promoting the objects of the institution, on this as well as on former occasions, the society is much indebted.

A ground rattan from the Ram Ghat in the western range, north of the parallel of Goa, and stated to be 225 feet in length, was presented by Lieut.-col. Blacker, through the medium of Major Macdonald.

The Hon. Sir E. Stanley moved the thanks of the society to the Hon. Sir John Newbolt, for his able exertions as President, which was unanimously carried, and the President returned thanks in a neat and impressive speech. The President having withdrawn, it was moved, and carried unanimously, That Sir John Newbolt, their first President, by whose able exertions the society had been established, and under whose fostering care it had attained so great a degree of success, be requested to continue President of the society, and that an acting President be elected at next meeting.

It affords us great pleasure to state, that the President having conveyed to the Hon. the Governor the unanimous wishes expressed at the last meeting of the society, Sir Thomas Munro was pleased accordingly to do the society the honour to accept the office of Patron, in the room of the late governor, the Right Hon. Hugh Elliott.—*Mad. Gov. Gaz. Sept.* 7.

 XXXVI. *In-*

XXXVI. *Intelligence and Miscellaneous Articles.*

PLATINUM.

The largest piece of this metal aver met with, weighs one pound and a half. It was found by a Negro slave in the gold mines of Condoto, in the government of Choco, and is now lodged in the Royal Museum in Madrid.

TEST FOR BARYTES AND STRONTIA.

Make a solution of the earth, whichever it may be, either in nitric, muriatic, or some other acid which will form a soluble salt with it: add solution of sulphate of soda in excess; filter, and then test the clear fluid by subcarbonate of potash. If any precipitate falls down, the earth was strontia; if the fluid remain clear, it was barytes.—(*Journal of Science and the Arts*, x. 189.)

SELENIUM.

The proprietors of the Sulphuric-Acid Works at Gripsholm, in Sweden, having had occasion to repair the leaden chamber of their manufactory, collected a few pounds weight of a substance chiefly consisting of sulphur impregnated with *Selenium*, which Professor Berzelius, of Stockholm, has forwarded to London, to the care of William Allen and Co. Plough-court, Lombard-street, requesting that it might be sold for the benefit of the proprietors of the Gripsholm Works, at a price which they have fixed. By application, as above, it may be had in bottles of the following sizes, together with a translation of the process recommended by Professor Berzelius for procuring this new substance:

		£	s.	d.
2-ounce phials, price	..	0	5	0
4-ounce —— ——	..	0	9	6
8-ounce —— ——	..	0	16	6
16-ounce —— ——	..	1	10	0

DYEING.

We announced some time ago that M. Braconnot, of Nancy, had discovered means for applying orpiment to stuffs of all kinds, and laid his process before our readers. M. L. Lassaigne has since succeeded in fixing chromate of lead with the fibre of all kinds of stuffs, by following a process similar to that employed by M. Raymond, to fix Prussian blue upon silk. The following is the process described by M. Lassaigne.

The well cleansed skeins of silk are immersed for a quarter of an hour in a weak solution of subacetate of lead at the common temperature; they are then taken out and rinsed thoroughly in a large quantity of water. This combines the silk with a certain

tain quantity of the subacetate. The silk thus prepared is afterwards immersed in a weak solution of chromate of potash, which causes it to assume a yellow colour, which continues to deepen more and more for about ten minutes, when it reaches its greatest intensity. The silk is then washed and dried.

A solution of native chromate of iron, decomposed by nitrate of potash, and saturated with nitric acid, may be employed in place of the neutral chromate of potash.

This colour is unalterable in the air; and by varying the proportions of subacetate of lead, and the chromate of potash, tints varying from pale to deep yellow may be obtained.

The process is applicable also to woollen, cotton, and linen; but with these the solution of subacetate should be heated to about 130° of Fahrenheit.

This dye however is liable to the inconvenience of being partially decomposed by soap: its use therefore had best be confined to silk.

MAGNETISM.

Professor Hansteen, of Christiana, has announced that he has ascertained that every perpendicular object, of whatever materials,—for instance, a tree, the wall of a house, &c.,—has a magnetic north pole at the foot, and a south pole at the top.—*Annals of Philosophy, N. S.* No. 2.

HEAT OF VACUUM.

M. Gay Lussac has lately proved by well contrived experiments, that when any portion of space, void of ponderable matter, is suddenly dilated or diminished, a thermometer, in such space, undergoes no sensible change. It appears plainly, that a vacuum, if it contain caloric at all, cannot contain it in the way that bodies do; and that the heat which irradiates through it instantaneously, is all that it holds, and is so infinitely small as not to be appretiable by instruments.

FINE ARTS.

We have great pleasure in announcing that a desideratum long called for has at length been gained, for giving to the public suitable convenience for viewing in a proper manner the productions of our engravers. It is now determined to open a regular exhibition of engravings of living British artists; and as the King has most graciously given the undertaking his Royal patronage, there can be no doubt of its success. The exhibition will open in the middle of April, under the direction of a superintending committee of engravers.

The very high state of perfection to which the art of engraving has been brought in Great Britain, and the ineffective modes in which

which its productions have been hitherto exhibited to the public, rendered it absolutely necessary that some means of doing better justice to the talents of the country, and of cultivating a more intimate connexion between the patrons of this fine art and its professors, should be devised.

A great portion of the time of British engravers has, of late years, been applied to the illustration of literature; but the most exquisite performances in this way are scarcely produced before they are locked up in the cabinets of the curious, never to be looked on but by a chosen few. The best proofs of works on a larger scale, meet with no better fate. These facts apply, in a greater or less degree, to every style of engraving; so that engravers themselves are, in many instances, unacquainted with what is passing out of their own departments. Such an institution as the present was therefore much to be desired; and we are happy to add that the plan has met with the highest encouragement from those of the nobility and gentry who are most conversant with the fine arts.

EUHARMONIC ORGAN AT CALCUTTA.

The friends of harmonic improvements will regret to learn, that the liberal views and intentions of the Elders and Congregation of the Scotch Church at Calcutta, which induced them to purchase one of Mr. Liston's improved Organs, and to take out Mr. *John Alsager* as their Organist *, are likely to be frustrated by the sudden death of that gentleman, from a stroke of apoplexy, which occasioned him to fall lifeless from his seat, while performing before the congregation! This melancholy event happened many months ago, and we regret to mention a report, which we hope will prove untrue, viz. that the elders, despairing of the opportunity of quickly supplying Mr. Alsager's place, had employed some organ-builder who is resident there, to cut down this fine and *unique Instrument* into a common organ, having only 12 sounds in its several octaves.

CICERO DE REPUBLICA.

After being buried for ages in obscurity, a copy of this tract, so long sought for in vain, has been found by M. Maio in the library of the Vatican.

VETERINARY ART.

The following recipe is given in an American paper, as an effectual cure for a horse having a film, as it is called, over his eyes.

"Take a little clean hog's lard on the end of your finger, rub it well in the quadruped's eye, once a day, for three or four days in succession, and the film will be removed effectually."

* See our 49th Volume, p. 266, and vol. 53, p. 395.

SCARLET FEVER.

It is announced in the *Journal de Medecine Pratique* of Berlin, that the *Belladonna* is a preservative against this fever. The fact was first discovered at Leipsic, but it has lately been confirmed by several experiments.

SNAKE WITH TWO HEADS.

Dr. Corradori, at Ruto, in Tuscany, lately saw a snake with two heads. Sometimes it happened that the heads differed as to the use of their faculties: thus the one head would eat while the other was asleep.

POTATOES.

Last year (says a writer in *The Farmer's Journal*), I planted a row of sets, cut out into single eyes, from large potatoes chosen out of a heap; the row was 25 yards in length: and next to it I planted another row of equal length, from the smallest potatoes, picked from the same heap: some of the latter were set whole, and some cut in half. When I took them up, the former row produced *four bushels and a half* of fine large potatoes, with scarcely any small ones. The other row gave so few in measure that they all went into a half-bushel scuttle, and were miserably small.

GEOGRAPHICAL INQUIRIES.

Count Romanzow has fitted out two new expeditions from Russia: one to endeavour to travel along the solid ice on the coast of Tschutski from Asia to America: the other to ascend one of the rivers on the N.W. coast, in order to penetrate the unknown space which is between Icy Cape and Mackenzie's River.

DANGERS IN THE RED SEA.

Mr. Editor,—Sir,—As I am convinced that you are ever ready to give publicity to any communication where the safety of lives and shipping is concerned, I shall, without apology, beg a place for the insertion of the undermentioned dangers, discovered by the Syren during her late voyage up and down the Red Sea. Jan. 24, 1820.—A reef extending north and south about 600 yards, lat. 20° 43′ N. and long. 37° 36′ E. by chronometers.

Jan. 31.—A reef in lat. 25° 12′ N. and long. 34° 56′ E. by chronometers.

April 20.—A reef in lat. 24° 45′ N. and long. 35° 8′ E. by chronometers.—A reef (a round spot), in lat. 24° 51′ N. and long. 35° 12′ E. by chronometers.

From

From the latter another of considerable extent bore W.S.W. five or six miles.—Observed three low islands, extending N.W. and S.E. about eight miles (not Grove Island, &c. &c.); the centre one is in lat. 24° 41′ N.—A reef in lat. 24° 35′ N. and long. 35° 25′ E. by chronometers: with another reef bearing from the latter W. ½ N. five miles.

April 21.—A reef in lat. 24° 8′ N. and long. 35° 45′ E. by chronometers.

The following is an extract from my log, relative to the shoal said to have been seen by the Fury and Dædalus, which may prove satisfactory:

Jan. 30.—At two P. M. saw a small sandy island from the mast-head, bearing N.E. to E. distant eight or nine miles, which appeared to be a quarter of a mile in length, with a reef extending to the northward from it, about a mile and a half, and to the southward a quarter of a mile.—I made it in lat. 24° 59′, and long. 35° 59′ E. by chronometers, whose rate had been found correct 22 hours before. Horsburg places this shoal in lat. 24° 58′ N. and long. 36° 56′ E. Capt. Court has laid it down in lat. 24° 56′ N. and long. 35° 49′ E.

As the north winds, not unfrequently, blow with such violence down the Sea of Suez, as to oblige a ship to bear up for Tor (the only safe harbour before known, I believe, between the straits of Jubal and Suez), and thereby causing a great delay and loss of time, I beg to recommend, as an excellent place of shelter, the Bay alluded to in the following extract from my log: *viz.*

Jan. 29.—At nine A. M. tacked in seven fathoms in a fine sandy bay, the extreme point of a reef of breakers, projecting from the land, forming the north side of the bay W. ¾ N. and a head-land forming the south side S.E. by S.—9. 30 A. M. tacked and stood in again; found the soundings gradually decrease from 10 to 9, 8, 7, 6½, and 6 fathoms, within half a cable's length of the shore: tacked and stood out.—The bay is in lat. 29° 12′ N. (on the Arabian shore), and would afford excellent shelter from N.W. or even W.N.W. winds; the holding ground is capital; and from the number of shrubs and trees growing near the beach, I think water might be procured by digging.

I am, Sir, yours, &c.
THOS. M'DONNELL,
Late Commander of the Syren.

Calcutta, June 20, 1820. (*India Gazette.*)

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To Henry Penneck, of the town of Penzance, in the county of Cornwall, Doctor of Physic, for improvement or improvements of machinery for the purpose of lessening the consumption of fuel in working steam-engines.—Dated the 27th February 1821.—6 months allowed to enrol specifications.

To William Frederick Collard, of Tottenham Court Road, in the parish of St. Pancras, Middlesex, musical instrument maker, for certain improvements on piano fortes.—8th March.—6 mo.

To Stephen Wilson, of Streatham, in the county of Surry, esq. for certain improvements in machinery for weaving figured goods.—8th March.—4 months.

To Robert Burton Cooper, of the Strand, in the county of Middlesex, sword-cutler, for improvements on or a substitute for stoppers, covers or lids, such as are used for bottles, tobacco- and snuff-boxes, ink-holders, and various other articles requiring stoppers, covers or lids.—3d March.—6 months.

To Jonathan Dickson, of Holland-street, Blackfriars, in the county of Surry, engineer, for improvements in the means of transmitting heat, and also in the means of transmitting cold from one body to another, whether solids or fluids.—5th March.—2 months.

To Henry Browne, of Derby, in the county of Derby, chemist, for improvements in the construction of boilers, whereby a considerable saving in fuel is effected and smoke rapidly consumed.—16th March.—4 months.

THE NEW COMET.

Observatory, Gosport, March 17.

The comet came to its perihelium to-day, namely, within 14 degrees of the sun. It has only lessened its right ascension half a degree, and its north declination four-fifths of a degree, since the evening of the 24th of February, when it was first seen here; but by the annual motion of the earth, its distance from the sun is decreased about 16°. Now it is advanced too far in the solar rays to allow us to make correct observations on its position.

At the close of this month it will begin to set after the sun, and with a clear horizon an hour before sunrise, there will be a chance of seeing it rise about E.N.E. during the ensuing month. The weather has lately been unfavourable for seeing the comet so near the western horizon in the evenings; as, from its very slow geocentric motion, it will not afford sufficient space to attempt to deduce the form of its orbit, which is the chief object to science.

It is hoped that correct observations on the frequent appearance of those celestial visitors, will, in the course of time, throw

new light on the theory of comets, and divest it of much of the uncertainty that seems to exist, in regard to the form of their eccentric orbits and their periodic returns.

VOLCANO IN THE MOON.

In the proceedings of the Royal Society (given in our last Number) it will be seen that on the 8th February the discovery of a volcano in the moon was announced to that distinguished body. By the following paragraph, copied from a Plymouth newspaper, the same or another volcano seems to have been observed also by another person on the 16th of January.

"Mr. Cooke, of Stonehouse, having constantly made observations on the moon for the last twelve months, discovered, about nine o'clock on the night of the 16th January (two days before the full, and the only bright night of the moon), an effusion of smoke, which lasted about a minute, and appeared like the fluttering of a bird. It passed over the moon before it evaporated, and must have fore-shortened, as it seemed in effect to have passed over the whole disc, from the place whence it arose, on the east of the spot *Menelaus* and near *Plineas*; but the effusion prevented the exact spot from being ascertained. Mr. Cooke had nearly finished a painting of the face of the moon in oil, seven feet in diameter, when he learnt from a friend in the neighbourhood, of the discovery of a volcano, which has induced him to delay it; but it is very likely the same."

QUERE RESPECTING TABLES OF VESTA.

To Mr. Tilloch.

Sir,—Give me leave, through the medium of your excellent publication, to avail myself of the opinion of some of your readers who may be fully instructed with respect to the management of M. Daussy's Tables of Vesta, inserted in the *Connoissance des Tems* for 1820. I had amused myself during the winter in calculating the orbit from January to April, and had a design of communicating it to your Magazine; but as Mess. Groombridge and Bode just at the moment published the positions of the planet, I deemed it necessary to transmit my computation. Upon comparing my positions, however, with those of Mr. Groombridge (for the meridian of Greenwich ?) I perceive mine to differ in almost every instance; and though the error in no case amounts to more than three minutes of a degree in right ascension, and sometimes only to a few seconds, yet, as I conceive the fault to be mine, I feel anxious to discover the cause. There are two notes, one at page 219, and the other page at 255 of the *Connoissance des Tems*, neither of which I clearly understand; and from this circumstance the defect probably arises. The former

mer note states, that in order to obtain the perturbations of the orbit with great exactness, it was necessary to carry the calculation to hundreds of seconds, which have been given in the Tables: "Mais on pourra pour l'usage ordinaire se contenter des dixiemes, et *retrancher en même tems un chiffre des tous les argumens.*"

I am at a loss how to interpret this *cutting off a figure from all the arguments.* Had it stated, *from the equations,* I should have concluded that, if the calculations were carried to two places of decimals, the last figure should be suppressed. But how a figure is to be cut off from all the *arguments* of longitude, except as is usual when the argument exceeds the extent of the Table, I do not see. The same difficulty occurs page 256, where it is observed, "La Table suivante donne les perturbations du rayon vecteur en dix-millioniémes; c'est pourquoi, après en avoir fait la somme, il faudra en retrancher le dernier chiffre, le rayon vecteur n'étant donné qu'en millioniémes." Now the extent of the Table of Equations of the Rad. Vect. to Argum. I. is 10000. I conceive therefore that the meaning of this note is, that when the equation exceeds four figures, the last (to the left hand) is to be suppressed. I do not know how it can be interpreted otherwise with propriety.

If any gentleman who has satisfied himself as to these difficulties, will be kind enough to communicate his sentiments to me through your Magazine, I shall esteem it a favour, and feel greatly obliged to him, and to you.

Permit me to ask if the Tables of Ceres, Pallas, and Juno, are printed? Bode gives the positions of the two latter occasionally, but I am ignorant whether the Tables are in private hands, or not.

March 7, 1821. Z. N.

BAROMETRIC MEASUREMENTS OF HEIGHTS.

Howland Street, 9th March, 1821.

Sir,—I have been much gratified by observing the zeal with which the efforts of *Mr. Bevan* have been seconded by your Correspondents, in communicating their Barometric Observations, carefully made in defined situations, on *the second Monday in each Month,* at the hours 8, 9, 10, 11, and 12 of the forenoon; and I sincerely hope and beg to request, that they will further persevere; and also that other gentlemen resident on the different Coasts of our Island will imitate their praiseworthy example; carefully connecting their place of observation, by a Spirit Levelling, with the *low and high water marks,* of their respective neighbourhoods. These maritime observations are of the utmost consequence to be simultaneously made with others at various inland points, towards obtaining nearly aproximating *heights* of

 the

the several places of observations, and what is perhaps of more importance, towards ascertaining, more exactly than is yet known, what are the causes which locally affect the atmospheric pressure? and under what circumstances may a uniformity of its action, as exhibited by the Barometer, be reasonably expected?

I beg, in conclusion, to entreat of those gentlemen who keep meteorological journals, to imitate the example of Mr. Cary, in p. 160 of your last number, by communicating exact observations, made by their Barometers, on the days and hours above specified: these more minute observations, for comparison with the monthly and other means from their tables, cannot fail of proving highly useful. J. FAREY, Sen.

P. S.—I am glad that Mr. Bevan has called the attention of your readers to the general inaccuracy of the Ordnance Trigonometrical *Heights:* the fact is, that paltry and insufficient instruments have been depended on *for Elevations;* used with less care, as to simultaneous and often repeated observations, than would have been proper, with other and far more efficient instruments: in p. 429 of your 48th volume, and on other occasions, I have before adverted to these circumstances.

BAROMETRICAL OBSERVATIONS.

26, Arlington-street, Camden Town, 12th March 1821

Hour.	Barom. inside.	Thermometers att.	Thermometers det.
8 Morning.	29·380	58.	42·
9 Do.	29·415	58.	43
10 Do.	29·415	58·5	46·
12 Do.	29·410	60·	51·5
3 Evening	29·350	62·5	52·
11 Do.	29·395	65·5	41·5

The difference of levels between Leighton and this place for 8 o'clock, by Laplace's formula with the coefficient 18393^m or 10057,6 fathoms, is .. .. feet 89,668 Diff.

By the method of Hutton ..	89,412-	—+0,144
For 12 o'clock, by Laplace's ..	109,656	
By Hutton's	109,536	——–0,120
The mean of the levels by Laplace's	99,462	
The difference between the observation at 8 and at 12 is	20,388	
By Hutton's, is	20,124	
The mean by Hutton's	99,474	
Difference	0,264	0,012

The

The observations made in different places, on the 1st Monday of the last four months, are not more satisfactory than the above. I find the mean difference between Leighton and Bushy-Heath 219,332 feet; but there remains a great uncertainty on the true difference of level: it amounts to 73,512 feet in February, in calculating the observations at 8 and at 12 o'clock, while in November it is only 3,216 feet from the observations at 9 and at 10.

Sir,— I send you the observations made at Arlington-street on the 12th of this month. The reflections which follow them are inserted merely for you, and Mr. Bevan, if you think that they may be interesting to him.

As I am going abroad at the end of this month, I shall not be able to furnish your Magazine with other meteorological observations made in Camden Town.

I am, sir, your most obedient servant,

To Mr. Tilloch. A Constant Reader.

Crumpsall, Lancashire, Mar. 13, 1821.

Sir,—I send you observations on the barometer, &c. made at this place on Monday the 12th instant.

1821.	A. M.	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
			°	°		
Mar. 12th	8	29.492	44	42·2	W.S.W. light.	Cloudy.
	9	29.500	45.7	44.7	W.S.W. Do.	Do.
	10	29.515	46.7	45.7	W.S.W. fresh.	Bright.
	11	29.520	48 5	47.5	W. by S. brisk.	Do.
	12	29.525	49·5	49	W. Do.	Do.

I observe that there is a typographical error in my former communication, published in the last Number of your Magazine, which it may be well to correct, as it affects the sense of the passage in which it occurs. The word "connection," in the fifth line from the top of the 154th page, should be read "correction."

Your obedient servant,

To Mr. Tilloch. John Blackwall.

Leighton, March 24, 1821.

Dear Sir,—I send you the observations made with the barometer on the 13th instant, at this place, and also at Bushy-Heath, by Col. Beaufoy.

LEIGH-

LEIGHTON BUZZARD.

1821. March 13.	Hour.	Barom.	Ther. att.	Ther. det.	Wind and Weather.		
	8^h	29·668	$43\frac{1}{2}$	40	W.	brisk.	
	9	29·685	$44\frac{1}{2}$	43	S.W.	moder.	thin clouds.
	10	29·692	46	45	W.	do.	a few clouds.
	11	29·692	$46\frac{1}{2}$	49	S.W.	do.	detached do.
	12	29·688	$47\frac{1}{2}$	51	S.W.	do.	cloudy.

BUSHY-HEATH.

	Barom.	Ther. att.	Ther. det.	Wind.		Weather.
8^h	29·463	43	38	W.S.W.	fresh.	thick fog.
9	29·464	43	40	W. by S.	do.	do.
10	29·474	$43\frac{1}{2}$	41	W. by S.	do.	do.
11	29·473	44	45	W. by S.	do	fine.
12	29·471	45	47	W.	do.	cloudy.

So far as the observations have at present been made, they point out some defects in the data relative to great distances, which probably may be supplied by a well regulated course of observations.

Good observers have often found an irregularity in the results when the weather has been *windy;* and if the cause of the wind be duly considered, there will appear some probability of the necessity for taking into the account the *direction* and *velocity* of the wind in addition to that of the heat of the mercury and of the surrounding air. Because, if there were not some local inequality in the pressure of the atmosphere, there would be no wind, to restore the equilibrium, any more than there would be a current in a lake without the addition or subtraction of a quantity to or from the primary bulk.

This inequality of pressure may be supposed to cause a wave on the superior parts of the atmosphere, requiring a suitable allowance to one of the instruments before a proper deduction can be made of the relative altitudes.

To ascertain the law of this allowance, it will require a good course of well arranged observations at places of known distances and difference of elevations under a variety of states of the wind, both as to direction and force. But a single course one day in a month cannot be expected to produce a satisfactory result in a reasonable time: it is to be hoped that some other course will be adopted. Yours truly,

To Mr. Tilloch. B. BEVAN.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

BY MR. SAMUEL VEALE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Feb. 15	13	38·	30·30	Cloudy
16	14	41·	30·40	Ditto
17	full	41·	30·18	Fine
18	16	39·5	30·05	Cloudy—rain P.M.
19	17	36·	30·27	Fine—snow A.M.
20	18	36·	30·	Rain—snow A.M.
21	19	43·	30·44	Cloudy
22	20	40·5	30·15	Fine
23	21	42·5	30·15	Cloudy
24	22	39·5	30·	Fine
25	23	40·	30·	Cloudy
26	24	34·	30·65	Ditto
27	25	7 A.M. 23· 1 P.M. 32·5	29·80	Fine
28	26	32·5	29·25	Cloudy—snow P.M.
Mar. 1	27	39·	29·35	Rain—snow A.M.
2	28	48·	29·68	Fine
3	29	52·5	29·50	Rain
4	new	52·	29·93	Cloudy—rain P.M.
5	1	35·	29·85	Ditto
6	2	36·5	29·60	Rain
7	3	51·	29·35	Fine—rain P.M.
8	4	54·5	29·87	Cloudy—heavy rain P.M.
9	5	54·	29·33	Fine—rain P.M.
10	6	57·	29·26	Ditto
11	7	52·	29·64	Ditto
12	8	50·5	29·66	Ditto
13	9	54·5	29·75	Ditto
14	10	49·	29·98	Ditto

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March 1821.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
Feb. 24	28	35	30	30.30	Foggy
25	32	43	37	.08	Cloudy
26	33	33	30	·12	Cloudy
27	25	37	29	29·81	Fair
28	30	32	37	·29	Snow
March 1	37	45	38	·42	Fair
2	38	47	46	·90	Rain
3	47	52	47	·62	Rain
4	48	55	46	·65	Fair
5	36	34	32	·98	Cloudy
6	34	38	45	·66	Rain
7	37	52	47	·61	Fair
8	47	51	46	·30	Rain
9	45	54	48	·65	Rain
10	49	57	47	·65	Showery
11	45	52	42	·87	Fair
12	42	56	46	30·00	Fair
13	42	55	44	·05	Fair
14	44	49	42	·23	Fair
15	35	53	40	·40	Fair
16	37	51	39	·30	Fair
17	37	50	40	·02	Fair
18	40	50	40	29·52	Stormy
19	39	47	39	·27	Fair
20	40	47	40	·34	Cloudy
21	39	47	39	·39	Showery
22	37	46	34	·81	Fr.with hialstorms
23	33	45	38	30·10	Fair
24	40	47	40	29·75	Stormy
25	43	51	38	·45	Showery
26	40	50	44	·66	Fair

N.B The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the
12th March 8 o'Clock M. Barom. 30·000 Ther. attached 45° Detached 42
— — — 9 — — — 30 002 — — — 46 — — 44
— — — 10 — — — 30·007 — — — 46 — — 46
— — — 1 — N. — 30 000 — — — 47 — — 56

XXXVII. *Observations on Statements made by Mr.* Ricardo, *and others, "On the comparative Advantages of illuminating by Gas produced from Oil and from Coal."* By Mr. George Lowe.

To Mr. Tilloch.

Dear Sir,— So great is the interest excited at this time, respecting the qualities of oil, during heating, boiling, or being converted into gas, that scarcely does there appear a single number of the scientific journals, in which the subject, in some shape or other, is not to be found amongst its contents. There can be little doubt, could we but visit the chemical laboratories throughout this kingdom, that we should, in not a few, find some *odoriferous* trace of its being, or having been, the subject of inquiry. So far then as the late memorable trials (which were at the time too truly styled "the humiliation of science") may tend to a patient investigation of experimental chemistry, prior to the delivery of any public opinion on them, they may eventually subserve the interests of science itself.

Our increased knowledge of the gaseous compounds, arising from the destructive distillation of oil and coal, is certainly much indebted to the late judicial investigations, as from hence have arisen numerous essays and papers, each inviting discussion on the subject, in connexion with our truly national invention of gas illumination.

The question arising out of these discussions, to which at this time I wish more particularly to draw the attention of your readers, is, "the comparative advantages of illuminating by gas produced from oil, and from coal," this being the title of a paper by Mr. Ricardo, which has just appeared in the 3rd number of the Annals of Philosophy.

So nearly allied is this attempt to run down coal gas, to the train of argument adopted by the Writer of an Essay on the same subject, which appeared in the 14th number of the Quarterly Journal (to which I replied in the 261st Number of the Philosophical Magazine), that even the expressions in many instances are alike; of course the conclusions drawn are identical. Whether or not the same hand which held a pen in the former was concerned in the paper now under examination, matters little, as I shall save myself and your readers the trouble of again discussing assertions unsupported by fresh arguments, by referring them to my former reply. In a word, I do not hesitate to maintain, that by far the greater part of Mr. R.'s conclusion against the use of coal gas in favour of oil gas, was drawn from distorted and erroneous data.

 Let

A.	Conn. des T.		Formula.		Diff.
°	′	″	′	″	″
45	00	58·2	00	58·2	0·0
60	1	40·6	1	40·6	0·0
70	2	38·8	2	38·7	− 0·1
80	5	19·8	5	19·6	− 0·2
85	9	54·3	9	53·3	− 1·0
86	11	48·3	11	46·4	− 1·9
87	14	28·1	14	24·5	− 3 6
88	18	22·2	18	15·8	− 6·4
89	24	21·2	24	10·6	−10·6
$89\frac{1}{4}$	26	20·4	26	, 8·5	−11·9
$89\frac{1}{2}$	28	32·1	28	21·9	−10·2
$89\frac{3}{4}$	31	1·8	30	53·0	− 8·8
90	33	46·3	33	46·6	+ 0·3

It appears from this comparison that the formula is in perfect agreement with the French Table, at least as far as 80° from the zenith, which includes all the useful part of such a Table. At 85° from the zenith the difference amounts only to 1″; and the greatest divergence at 0° 45′ above the horizon is short of 12″.

At 70° from the zenith the third term of the series is insensible; and at 80°, it only comes to 0″·19; so that, if we neglect this small quantity, the two first terms of the series are sufficient for all stars elevated 10° above the horizon. The three first terms are sufficient as far as 85° from the zenith.

Most physical problems are solved by a series of attempts in which some of the conditions are either omitted entirely, or so modified as to bring the investigation within the range of our knowledge. The first attempt to solve the problem of the astronomical refractions was made by Cassini, who neglected all changes of density in the atmosphere, whether arising from unequal pressure or variation of temperature. In this view, the air would constitute a uniform refracting medium surrounding the earth to the height of about 4543 fathoms. The simple hypothesis of Cassini seems hardly to have met from astronomers with the attention it deserves: for, if we use accurate elementary quantities in the computation, it will determine the refractions to the extent of 74° from the zenith with the same degree of exactness as any of the other methods, without even excepting the formula of Laplace. In an atmosphere such as Cassini supposed,

ture. "It may, I think, be stated with sufficient accuracy for practical purposes, that a gasometer containing 1000 cubic feet of oil gas is adequate to furnish the same quantity of light as one of 3000 of coal gas, provided due attention be paid to the construction of the burners and to the distribution of the lights." How the Professor could possibly *think* this, when even his own calculations (erroneous as they may be readily proved) did not bear him out by a deficiency of above 20 per cent., I know not. The title of the paper from which this extract has been made, is "On the Composition and Analysis of the inflammable gaseous Compounds resulting from the destructive Distillation of Coal and Oil, with some Remarks on their relative heating and illuminating Powers, by William Thomas Brande, Esq. Sec. R. S. Professor of Chemistry R. I." published in the Philosophical Transactions for 1820, Part I., and in the Phil. Mag. for last September.

The chief object of this paper was to prove that no other gaseous compound of carbon and hydrogen exists, but the one usually called *olefiant* gas; the second section institutes a comparison of the illuminating and heating powers of olefiant gas, oil gas, and coal gas. How far he has succeeded in the undertaking, may be partly ascertained by referring your readers to a review of it by Professor Thomson in the 95th Number of the Annals of Philosophy, and to the 68th Number of the Edinburgh Review. A very short extract from each, will be sufficient to put us in possession of the *material errors* from which Mr. Brande's ratios against coal gas are deduced. Thomson observes, "His specific gravities and atomic weights are without any exception inaccurate; I am surprised at the low specific gravity of coal gas, which he assigns, viz. 0·4430," which would serve most completely to run down coal gas in all his after calculations.

Dr. Henry found the specific gravity of coal gas of medium quality to be ·622, and of that produced from coal tar to be as high as ·780. The latter portions of coal gas, when made on the old principles of long charges, of course are much lower, sometimes as low as ·390; but where three or four hours' charges are adopted (as at Derby), or the tar converted into gas, which for the last year I have found very practicable, and which will soon be heard of on a very extended scale, the gas will be found to be so far increased in illuminating power as generally to average from ·550 to ·600.

From this it is evident that the coal gas will vary according to a good or a bad method of manufacture, and all calculations of comparison like Professor Brande's or Mr. Ricardo's, be thus in a like ratio affected. The Edinburgh Review says, "We must take leave to observe, that in some parts of his inquiry, Mr.

Brande's train of reasoning is a little fallacious;" and as to his arithmetic, they are (like myself) quite unable to comprehend it. Nevertheless, the points upon which we stumble are not exactly the same, the reviewer himself being evidently at fault when he conceives that the number of cubic inches of olefiant gas (640) and of oil gas (800) found equal to the light of one wax candle per hour, should *each* be multiplied by the same multiplier (14·0625) to increase the light ten-fold; because that in the instance of the *olefiant gas* it so required it. I feel very little doubt that the increase of the multiplier will be found to be in the inverse ratio of the decrease of carbon, which these gases may contain, still dependent upon the discovery of Count Rumford, "that the quantity of light emitted by a given portion of inflammable matter in combustion, is proportional in some high ratio to the elevation of temperature."

Now how will this agree with Mr. Brande's ratio for coal gas? It seems he forgot to ascertain how much coal gas was equal to the light of *one* wax candle, but sets about ascertaining the quantity of light given off by the largest sized Argand burner when consuming only 6560 cubic inches (about 3¾ cubic feet) per hour, which he found equal to only five wax candles. Therefore to obtain by calculation how much coal gas would be equal to *ten* wax candles, he sets down $6560 \div 5 = 1312 =$ the light of one candle, which multiplied by $10 = 13120 = 10$ candles! quite forgetting the Count's theory and his own practice with respect to the cases of the olefiant and oil gases. Now the fact is, that a large Argand when consuming only 3¾ feet per hour, instead of 5 feet, is not giving off any thing near so much light as the same quantity would if burnt in a smaller and more proportionate sized one *.

From such a datum so multiplied it is not in the least strange that coal gas of the sp. grav. ·443 should cut so poor a figure by

* The reason of this is best proved by experiment, to be, that the supply of atmospheric air caused by the draught of the glass chimney, is highly disproportioned to the quantity of gas in such case consumed, thereby exerting a very cooling influence on the flame. Had Mr. B. been aware of this fact, he doubtless would have adopted some means for its remedy: such as narrowing the draught by applying a taper chimney, or by placing over it a piece of coarse wire-gauze, or a disc of tin with a smaller opening in its centre than that of the glass chimney, either of which, so regulated as just to allow the flame not to smoke, would have materially increased the light of his 6560 cubic inches of gas. Surely Mr. Brande does not mean to say that in his experiment the flame was so regulated as to be just below smoking? for with coal gas, rich in carbon, I find that the same burner as his, under the same pressure of half inch, will not smoke when consuming five feet per hour and giving a light equal to eight or nine wax candles. Mr. B.'s 6560 inches would have given more light if burnt through either a bat's wing burner or a large Argand without a glass.

the

the side of oil gas as the Professor makes it appear. He says, "it appears (from his method of calculation) that to produce the light of ten wax candles for one hour, there will be required

2600	cubical inches of	olefiant gas.
4875	.. ..	of oil gas.
13120	.. ..	of coal gas."

As well might he have said, on the same principle, that because it requires 800 cubic inches of oil gas to equal the light of one candle, therefore it requires 8000 cubic inches to produce the light of ten candles! Had he multiplied 1312 by 8, as he did the oil gas by 6, or the olefiant by 4, the product 10496 (= 6·074 feet) would have been much nearer the truth. Their ratios then would have been

Olefiant gas	2600	ratio	1.
Oil gas	4875	..	1·875.
Coal gas	10496	..	4·037.

Numbers so little short of 1. 2. 4, that for practical purposes, where good coal gas is to be obtained, we may rest assured that two cubical feet of it, properly applied, will be found equal in illuminating power to one of oil gas*.

Before we quit this part of our subject, it may not be amiss to hint at one other source of discrepancy too frequently overlooked in the analysis of these gases, that of experimenting upon the gases immediately after their production, or after they have been made some time and transmitted to a great distance, perhaps miles (as was the coal gas in Mr. Brande's case †) through a series of cold pipes, whereby the deposition of a highly volatile oil takes place, which in the former case would have added greatly to its illuminating power, to its specific gravity, and of course to the quantity of oxygen it would consume.

Having now reviewed with some minuteness, yet I trust with candour, the theoretical basis of Mr. Ricardo's comparative statement, "that 20 cubic feet of oil gas will give as much light as 70 of coal gas," I will endeavour to travel through the other stages of his paper at a quickened pace.

* After Mr. Lowe's paper was in the hands of the printer, I received a letter from him containing the following paragraph:

"My chemical library having this day received the very valuable acquisition of Dr. Andrew Ure's Dictionary of Chemistry. I hope it is yet in the Editor's power to add (as a note) the weight of testimony to my argument, which the Doctor, under the article 'Oil Gas,' affords me; he says '*The oil gas I have been accustomed to make has only a double illuminating power compared to good coal gas.*' I should be sorry that Mr. Ricardo or your readers should lack evidence like this."—Edit.

† The oil gas made use of in Mr. Brande's experiments was manufactured in the Institution; but the coal gas came from the Gas-work, Peter-street, Westminster.

Theory

Theory we have quite done with; all is now matter of fact, or rather of assertions taken by Mr. R. as matter of fact: and in our endeavour to come to a due appreciation of these facts, we will endeavour to answer the most prominent points of his objections against coal gas, by adducing substantial facts which are daily before the eyes of thousands. "Oil gas (he says) requires no purification;" yet in the very next page, in describing an oil gas apparatus, he says "the oil gas having passed through the condenser, is then conveyed into a wash vessel, where it passes through water to deposit any oil, or other condensible vapour that may have come over with it."

To wash then, it seems, is not to purify! Now to manufacturers of oil gas, and still more to their neighbours, it is known that this said washing water is the vilest nuisance that ever passed down a sewer; in more instances than one, actions have been threatened on account of this no-purifying process. "It contains no sulphuretted hydrogen, which is one of the admixtures of coal gas, and of this all the purification to which it is submitted cannot wholly deprive it;" therefore, to make short, does every thing that is bad to our health and property, which of course oil gas does not. One feels very much inclined to ask, where, or in what town, did Mr. Ricardo find this fact, as a general thing? Certainly not in London; for I have there enjoyed its light in rooms the most elegantly furnished, where both pictures and plate, had its effects been deleterious, would have been assuredly injured.

As to the assertions of Dr. Henry, that it is easy to purify coal gas so that it shall not test $\frac{1}{20.000}$dth part of sulphuretted hydrogen, why surely that most accurate chemist must have been mistaken; but then I cannot doubt the evidence of the three last years in our own house, every room of which, not excepting bedrooms, proves the possibility of purifying coal gas. As to the accidental escape of coal gas being *more* annoying than a similar escape of oil gas, I think we need not waste words to prove, that, for the same reason, it is *less* dangerous. Mr. R. forgets to mention that acetic acid is sometimes formed in making oil gas, when he talks of the pipes stopping up. To satisfy myself, I last week cut open a copper tube which has conveyed more coal gas than any other pipe about our premises, but found not the slightest deposit in it! So much for Mr. R.'s first division, "The qualities of the two gases for producing light."

The second point for consideration is "the comparative facility with which a coal or oil gas establishment may be carried on." Hitherto I have endeavoured to go along with Mr. Ricardo with as grave a face as possible; but if, at the conclusion of this division, you, Mr. Editor, your reader, and even Mr. Ricardo himself,

himself, do not smile to think how he has been cheated in his numerals, I shall indeed be surprised. His own calculations will be quite sufficient to show how erroneous must have been his data.

After tracing the outline of each apparatus, he says, "These are the processes required for producing the oil and coal gas; but we shall better understand the trouble attendant on the latter, by a comparative view of two establishments, for a thousand lights each, one for oil, the other for coal gas. Each light consuming annually upon an average 2000 cube feet of oil gas, and 7000 cube feet of coal gas, the whole annual consumption of the one would be 2,000,000, of the other 7,000,000 cube feet." "The average quantity of oil gas required would be during the fortnight before and after Christmas, somewhat above 16,000 cube feet per night, and 56,000 of coal gas." That is, if every burner of the 1000 coal gas lights consumed five cubic feet per hour (which is found to be above the average for the town of Derby), the inhabitants would be required to keep them burning each night *eleven hours!* i. e. from four in the afternoon till three o'clock next morning!! an event just as probable as the calculation upon which it is founded, or as the truth of the stated number of retorts presumed requisite to furnish such a supply of gas. "To produce that quantity of oil gas, eight or ten retorts would be sufficient, each retort six feet long and six inches diameter."—"In the coal-gas work forty retorts at least would be required *, each retort six feet long and one foot in diameter. The utmost quantity of gas which one retort could produce would be 1680 cube feet, working night and day without intermission." —"*Forty retorts required to supply* 1000 *lights!!!!*" Six retorts is the calculation of Mr. Wigston at the Derby Gas-works, for the like number of lights; and from the experience of the three last months' work he finds they will be amply sufficient. His retorts are semi-elliptical, six feet long and two feet diameter, being constructed on the very best principles of exposing a thin stratum of coal to a large surface of heat, and working at short charges.

Surely Mr. Ricardo must imagine that no improvements have taken place in the manufacturing of coal gas for the last five years; whilst in that of the oil gas it should seem one gallon of oil which formerly made 80 cubic feet of gas will now afford 100. But even on the London system of setting up retorts, five in one

* For the truth of this statement Mr. R. makes a marginal reference to "See Peckston on Gas Lights." Now, in justice to Mr. Peckston, it is but fair to state, that he gives that number as being the quantity then (in 1818) required by the London Companies; for, in his estimate last year, to supply 1000 lights in Derby, he states ten elliptical retorts as being amply sufficient.

oven,

oven, as adopted at Nottingham, I understand that 34 circular retorts of the same size as those mentioned by Mr. R., have through this winter supplied above 1600 lights! each light extravagantly supplied with gas. So much for the correctness of the statement under Mr. R.'s second head.

His third head, "The capital required for such establishments," is involved in his fourth, which we will hasten to notice. He says, "The next subject for consideration is, Which is the more œconomical method? And it may appear surprising to many, that light from oil gas can be afforded to the consumer as cheap as that from coal; and at the same time yield as great or greater profit to the company supplying it." Indeed it will appear *very surprising* to many, especially to the Derby people, that it should be possible, after contrasting a *counter statement*. He says, "From some known data, it is supposed that the cost of coal gas to the Companies, reckoning the sale of coke, tar, &c. would be about 10*s*. per 1000 cube feet; the selling price is estimated at the rate of 15*s*."—"The profit and loss account of an oil gas establishment may be calculated very easily." "The cost for producing 1000 cube feet of oil gas will be as follows:

	£	*s*.	*d*.
Ten gallons of oil at 2*s*. per gallon	1	0	0
One bushel of coals	0	1	6
Labour, wear, tear, and contingencies	0	5	6
	£1	7	0"

This then (or rather 30*s*. which Mr. R. allows) is the *prime cost* of 1000 feet of oil gas, where the oil can be bought at the very cheapest rate, and no inland carriage to be added to it. What it must be charged by any public company to the consumers of it, so as to meet the many looked for and unlooked for expenses upon the sale thereof, and yet to divide 10 per cent. profit, I think would be difficult, at least disheartening, to say; inasmuch as it is just 100 per cent. dearer than its equivalent in coal gas may be *purchased* for in this town.

For the contract entered into with Mr. Wigston to furnish coal gas of the best quality into the Derby Gas Company's gasometer, he finding retorts, condenser, coals, lime, labour, wear and tear, is at the rate of *one shilling and eightpence* * per thousand feet!! and the price charged by the Company to consumers, who take it by measure, is *seven shillings and sixpence* per thousand feet!!!

* This I am aware is a price so low that it astonishes all, and is incredible to many. It shows that he is either content with very little profit, or that he has stept out of the path of the old method of making coal gas. In the construction and arrangement of some parts of the Derby apparatus he has certainly shown considerable mechanical ingenuity and independence of thought.

feet!!! The bare statement of these facts will, I trust, furnish the best comments upon this part of the "Comparative Advantages of illuminating by Gas from Oil and from Coal."

His fifth and last head, "Which is most durable in a national point of view?" is best answered by a general axiom, That that is the best source of a nation's revenue which is most lucrative, least fortuitous, and least hazardous. That oil gas should be preferred to coal gas, because it would thereby encourage the Greenland fisheries as a nursery for our fleets, is a rather strange doctrine, when we recollect that during war it is protected by law from that most disgraceful system, the impress! And we certainly have yet to learn how it can be put in comparison with the Newcastle coal trade, and that carried on along our eastern and southern shores. As I said in my former reply to the very same argument, I repeat my opinion, that where the former brings up one sailor, these latter sources bring up fifty.

We have now contemplated the *five-point* picture of oil gas, which the fanciful pen of Mr. Ricardo has presented to the reader of the Annals of Philosophy. Be assured, Mr. Editor, that in my thus attempting to hold up the mirror of truth, in order to take a fair impartial view of it, I am as free as even Mr. Ricardo himself from being "influenced by interested motives in seeking publicity to it." The cause of truth and of science is alone the object I am desirous to subserve: if Mr. Ricardo or any of your numerous readers should by the aid of *these reflections* discover that his chief points of "Comparative Advantages," highly coloured as they are, are out of all drawing, due keeping and proportion, and that, in order to make them bear out, not a few false shades have been thrown over coal gas,—then indeed shall I rest satisfied that I have neither written, nor caused you to print, in vain. Believe me, most respectfully,

Your friend,

Derby Brewery, March 21, 1821. GEO. LOWE.

XXXVIII. *On the Errors in Longitude as determined by Chronometers at Sea, arising from the Action of the Iron in the Ships upon the Chronometers.* By GEORGE FISHER, *Esq.* Communicated *by* JOHN BARROW, *Esq. F.R.S.**

THE determination of the longitude at sea by time-keepers, is so exceedingly easy from the simplicity of the observations and calculations employed, and from the general practicability of the method, as to render chronometers, in the present improved state of navigation, almost indispensable articles in the equipment of ships for foreign service; and I shall feel happy if the following

* From the Transactions of the Royal Society for 1820. Part II.

observations may, in any way, contribute to the more accurate determination of the longitude by this method.

The sudden alteration in the rates of chronometers when taken on board of ships, has been frequently observed by intelligent seamen; and is generally ascribed to the motion of the vessels. Before, however, I attempt to account for this alteration, I shall first prove that it actually takes place; and, in order to do this, shall relate the circumstances connected with the chronometers on board the Dorothea and Trent, commanded by Capt Buchan, which occurred during the late voyage to the north pole.

Soon after the arrival of the ships on the coast of Spitzbergen, the chronometers on board the Dorothea (five in number) were found to be rapidly gaining on their former rates as determined in London previous to the ships sailing; in consequence of which the land appeared considerably to the westward of its true position as determined by lunar observation, and they were found to be still gaining daily, which appeared not only from each subsequent set of lunars, but also by comparing the longitude of different points of land determined by the chronometers, with the longitude of the same points ascertained in the same way some time afterwards.

For instance: The longitude of a remarkable point of land on the north-west coast of Spitzbergen, called Cloven Cliff, was found by a mean of the observations taken with the chronometers on June 21, 1818, to be 10° 35′ 27″ E.; but the longitude of the same point of land on July 31, was 10° 15′ 37″ E., making a difference of no less than about 20° of longitude in five weeks; that is, estimating the longitude with the same rates and errors as determined in London before their departure. From this, therefore, it appears, they had been gaining on their former rates, or had been increasing their gaining rates, and diminishing their losing ones.

An opportunity soon afterwards occurred of observing the effect produced upon the chronometers by removing them on shore. On the 9th of August, the chronometers, nine in number, were landed on an island, where a temporary observatory had been erected for the purpose, and the latitude of which had been accurately determined with a repeating circle made by Troughton, when it was found that the acceleration immediately ceased; for the longitude of the place by chronometers, August 12, was 9° 42′ 36″ E.; but on the 27th, it was 10° 1′ 0″ E., making a difference of 18′ 24″ of longitude in fifteen days, using the former rates.

Since, therefore, the chronometers were *getting easterly* by their removal on shore, the acceleration must have ceased; which will appear upon consideration.

A similar circumstance was observed by Lieut. Franklin to take

take place with the chronometers on board the Trent, which were four in number; and he observes, "It may be worthy of remark, that the chronometers taken out by the Hon. Capt. Phipps showed too great westerly longitude, and consequently gained on these seas. The fact of so many chronometers altering their rates the same way, is curious; but I am not aware that any cause can be assigned."

The effect produced upon one or two of the chronometers by their removal to land, was very remarkable; a chronometer made by Baird was (by observations taken on shore near where the ships lay at anchor, by Lieutenants Franklin and Beechey, with false horizon, and eight-inch reflecting circles of Troughton, from August 8 A.M. to 12 P.M.) losing 3·″4 daily when on board; but upon its removal on shore to the observatory, its rate per transit, from August 16 to 26, was observed by myself to be 18·″2 losing. Upon again removing it on board, it was found by observation, as before, to be losing 6·″5 daily; from which it appears the chronometer lost no less than about thirteen or fourteen seconds daily by its removal on shore.

Another chronometer in the Trent, made by Pennington, had been gaining rapidly on board; when taken on shore, it acquired immediately a losing rate of 1·″8, nearly the same as it had in London before the vessels sailed.

A chronometer of my own, by Arnold, was affected likewise nearly as much, losing about 9″ daily by its removal on shore.

In the other chronometers the alteration was less sudden, but was ultimately not less considerable; and they were several days on shore before they acquired a steady rate, as will appear by the following table of their rates, during the interval of the respective dates.

Table of Rates of Chronometers immediately when landed, August 9, 1818.

	No. 1. Earnshaw.	2. Arnold.	4. Barraud.	5. Arnold.	Clock.
August 9	+12″·0	−0″·2	+8″·1	−33″·5	
11	+10 2	0·	+7·9	−34·2	
12	+ 7·98	−1·85	+0·8	−36·2	+70″·60
16	+ 6·2	−5·1	+7·1	−40·9	+69·03
20	+ 5·63	−5·2	+2·8	−37·7	+69·12
23	+ 4·02	−6·14	+4·3	−41·5	+69·29
26					

 The

The rates from August 12 to 26, were determined by the sun's transit, and those from August 9th (the day on which the chronometers were landed) to the 12th, by a comparison with the clock, supposing its mean rate +69″·5, as no observation occurred during this interval; and by this table it appears, that the chronometers when landed were rapidly diminishing their gaining rates, and increasing their losing ones. In the others the effect was almost immediate.

The clock and chronometers were likewise landed upon a small island in Fair Haven, on the north coast of Spitzbergen, on the 30th June; and, as the same circumstances occurred, it will be needless to detail them.

The following table is intended to show the difference between the rates on board the ships and what they would have been had they been on shore.

Chronometers.	Error, Greenwich time.	Difference.	Interval.	Mean Rates at Sea.	Mean Rates on Shore.
No. 1. Earnshaw.	April 11, + 7′.40″·1 Aug. 25, +26.32·2	+18′.52″·1	days 136	+ 8″·0	+ 3″·8
2. Arnold.	April 11, − 0.42 Aug. 25, − 4.15	− 3.33	136	− 1·5	− 5·2
3. Arnold.	May 7, − 1.44 July 2, − 3.36	− 1.52	56	− 2·0	− 6·5
4. Barraud.	April 1, − 0. 1 Aug. 25, + 5.55·5	+ 5·56·5	146	+,2·5	+ 1·2
5. Arnold.	April 15, − 0.38 Aug. 25, −28.48	−28.10	132	−12·8	−22·4
6. Earnshaw.	April 11, + 1.13 Aug. 25, + 0. 2·2	− 1.10·8	136	− 0·5	− 0·93
7. Pennington.	April 11, + 0.53 Aug. 25, +13.57	+13. 3·5	136	+ 5·8	− 0·63
8. Arnold.	April 13, − 0.24 Aug. 25, −15 53·5	+16.17·5	134	+ 7·3	− 2·5
9. Baird.	April 15, + 0.25·1 Aug. 25, + 5.12·5	+ 4.47·4	132	+ 2·2	− 5·15

The errors of the chronometers in April, were those obtained in London before the ships sailed; those on the 25th of August were determined at the observatory on Dane's Island, Spitzber-

gen, the longitude of which was determined by a great many observations of the distances of the sun and moon for several days with Troughton's eight-inch sextants and reflecting circles. The rates in the column entitled "Mean Rates at Sea," are deduced by dividing the difference of the errors by the interval.

The rates in the column entitled "Mean Rates on Shore," or more properly what they *would have had*, are means between the rates of chronometers on shore before leaving England, and those obtained at Spitzbergen; and although a mean between the rates of chronometers obtained at different times, may not accurately be the mean rate they would have had during the interval of those times, from the continued variation to which they are subject; yet, upon comparing the two last columns together, of the rates thus deduced, it will be perceived, that in all the chronometers their gaining rates had either been increased, or their losing ones diminished on ship-board, or, in other words, they had all been accelerated.

Nor is this acceleration peculiar to high latitudes; it was observed very soon after the chronometers were put on board in the River, particularly in Nos. 3 and 8, which, upon arriving at Shetland, were found to have gained instead of losing rates, which they had in London.

This acceleration was very soon perceptible in the chronometers taken out by the Hon. Captain Phipps, made by Kendal and Arnold. Mr. Lyons, who accompanied him, landed at Sheerness Fort, and found the longitude by them to be 30′ 0″ E. which is about 13′ W. of the true longitude, as determined in the Trigonometrical Survey.

The same occurrence took place last summer (1819). The longitude of a place in one of the Orkney Islands, as determined by three chronometers made by Arnold, two of them belonging to myself, the other to Lieut. E. Home, R. N., who accompanied me, was 6′ 40″ W. of the longitude determined by the difference of Æ of stars E. and W. of ☽.

Again, in the trial of Mr. Harrison's time-keeper, in 1764, the longitude of Barbadoes by the watch was 10′ 45″ more to the westward than that determined by astronomical observations made by the persons sent out for that purpose.

Soon after this trial, the commissioners of longitude agreed with Mr. Kendal, one of the watchmakers appointed by them to receive Mr. Harrison's discoveries, to make another watch on the same construction, which went considerably better than Mr. Harrison's. Mr. Kendal's watch was sent out with Captain Cook in his second voyage towards the south pole and round the world, in the years 1772-3-4 and 5, "when the only fault found

found in the watch was, that its rate of going was continually accelerated."

It now remains, therefore, to determine what this acceleration arises from. That it does not arise from the motion of the vessels, is evident in the case of the nine chronometers on board the Dorothea and Trent; since the acceleration was observed when the ships were firmly beset with ice; also in the case of the alteration in the rates of the chronometers upon landing, and taking them on board again at Dane's Island, the ships were riding at anchor close in shore without any perceptible motion.

An account was also kept on board and on shore, of the state of the temperature and barometer, every two hours, both night and day; and upon comparing them together, there does not appear to be the least correspondence between the change of rates and the temperature at the time.

It appears therefore to me, that this acceleration arose entirely from the magnetic action exerted by the iron in the ship on the inner rim of the balance, which is made of steel.

That the iron in the ships becomes magnetic, is plain, from the polarity which exists in it; the whole forming altogether one large magnet, having its south pole on deck nearly amidships, and its north pole below. This is seen from the constant deviation of the north end of the compasses placed on deck towards the centre of the ship, as appears from recent observations, which I have mentioned elsewhere.

Nor is it surprising that the force exerted by the ship's iron (thus become magnetical) on the balance of the chronometers, should be sufficient to cause a very sensible alteration in the rate of going, when we consider how easily, in other cases, the presence of any thing magnetical is detected by the alteration of the rate of a chronometer; and when we consider the great influence exerted by this iron upon the binnacle compasses at very considerable distances, and in situations where the utmost precaution is used to remove every piece of iron from them, by using copper-bolts, fastenings, &c.

It remains only to determine, how far this alteration in the rates of the chronometers, can be reconciled with that observed in chronometers when under the influence of magnets placed in different positions with respect to their balances.

To determine this, two watches were used, with steel balances and horizontal escapements, one by Earnshaw, the other by Allan and Caithness; also two chronometers made by Arnold. To each of these watches were applied, at a distance of two inches from the balance, magnets of twelve inches in length, in four different positions, and in the planes of the balances.

The

The following Table will show the rates of the watches in twenty-four hours, deduced by comparing them with an excellent clock with Graham's dead beat escapement, and regulated by transit.

	No. 1.	No. 2.			No. 3.	No. 4.
N S	+ 9.15 + 8.12	+ 5. 0 +21. 0	3	N S	+ 0.34 + 2.18	+ 0.41 + 0.44
N S	+ 8. 0 +48. 0	+18. 0 + 8. 0	6	N S	+ 1.28 + 5.27	− 1.43 − 0.36
N S	+47.10 +72. 0	+17. 8 + 8.44	9	N S	+ 5.22 − 0.14	+ 1. 3 + 0.41
N S	+ 4.14 − 2. 0	+ 4.32 +15. 0	12	N S	+ 2.47 − 1. 2	+ 1.12 + 1.24

The first column in this table shows the pole of the magnet applied to the watch; the second and third, the rate or effect produced on each watch; the fourth column shows the figure on the face of the watch opposite to which the magnet was applied.

The watch, No. 1, gained with both poles, and in every position of the magnet but one. No. 2 gained with both poles in every position. Nos. 3 and 4 gained in every position but two; and the quantities lost in the positions were far exceeded by the accelerations caused by the opposite poles, excepting one case in that of No. 4.

The magnets were likewise placed in different positions out of the planes of the balances; the results were very *similar* to those above, but differing in quantity, according to the distance of the magnets from the planes of the balances.

Upon placing the magnets very near to the rim of the balances, a very rapid acceleration took place with both poles, and in every position of the magnets, particularly in the watches Nos. 1 and 2. Upon too near an approach of the magnets, the watch No. 1, and chronometer No. 3, were rendered useless; the former, when the magnets were taken away, gaining no less than about $1\frac{1}{4}$ hour, and the chronometer losing about 50″ in 24 hours; and in again repeating the experiments in the plane of the balances, the rates of the chronometers (without the magnets) were so variable, that it was necessary to determine their rates before and after each application of the magnets: the following, however, is a Table of the results upon the chronometers Nos. 3 and 4.

No. 3.

	No. 3.	No. 4.	
N S	+ 0′. 2″·7 + 1.24	+ 0′.43″·5 + 0.46	3
N S	− 5.32 + 6.15	− 1.40 − 1.37	6
N S	+ 5.19 − 1.53	+ 1.18 + 1.12	9
N S	+ 1.59 + 2. 7·3	+ 1.11·7 + 1.29	12
N S	+ 0. 31 + 2. 8	+ 1.14 + 0.43	3
N S	− 5. 9 + 4.25·7	− 1.24 − 0.59	6
N S	+ 5.25 − 1.53	+ 0.55 + 0.42	9
N S	+ 2.33 − 2.23	+ 1.5 + 1.6	12

Each of these results in this Table, is the difference of the rates when the magnets were applied, and a mean of the rates of the chronometers before and after the application of the magnets. The rate of the chronometer No. 3, is very different from that given in the former Table; that of No. 4 is nearly the same, and does not appear to have been affected, as No. 3 was, by the close approach of the magnet.

Upon the whole, however, it appears that chronometers will be generally accelerated (particularly if their balances have not received polarity by the too near approach of any thing magnetical) on ship-board. It appears probable, likewise, that the force of the balance springs is affected in the same way; since it is well known that chronometers having gold balance springs, although more difficult to adjust, yet keep better rates at sea than the others.

However this may be, these observations show the necessity of not trusting to the rates of chronometers ascertained during the time

time they are on shore; and if the rates are ascertained on board, the chronometers should always be kept in the same place, and also in the same position with respect to the ship; for I have but little doubt that, upon an accurate trial, a chronometer will be found to change its rate, more or less, according as these circumstances are attended to. If these precautions are not taken, land will appear to be considerably to the westward of its true position: this is particularly exemplified in the observations of the Hon. Captain Phipps; from which nearly the whole line of coast on the west side of East Greenland has been placed nearly 1½° too much to the westward, by reason of the acceleration of his chronometers: the same circumstance would have occurred with the chronometers in both of the ships Dorothea and Trent, in the late voyage, had not the longitude been otherwise determined. It is therefore highly requisite that attention should be paid to a circumstance so much connected with the improvement of geography as well as the safety of the seaman.

The foregoing paper is followed by an Appendix containing Tables of Rates furnished by Mr. Coleman, teacher of navigation, and embrace observed rates in different voyages from 1802 to 1820 inclusive, corresponding very much with Mr. Fisher's, and tending to confirm his general inferences.

XXXIX. *Sequel of the Experiments on the Action of the Voltaic Pile upon the Magnetic Needle. By M.* BOISGERAUD *Jun.*

[Concluded from p. 206.]

I THINK it useful to make known an illusory phænomenon which the Voltaic apparatus I made use of presented. I took a very fine silk thread; I attached it to a fixed point by a small particle of wax. I took another very small particle of wax, which I fixed to the other end of the thread, in such a manner as to form a pendulum with it. I afterwards pressed the latter piece of wax on one of the poles of a magnetised steel wire which remained suspended. The other pole was rubbed with a little grease, so that I could make adhere to it horizontally a small needle of Spanish wax or silver. This arrangement, it will be observed, has some analogy with the electroscope of Coulomb. The object of it was to indicate the action of the conducting wire on the faces of the needle. The following are the deceptive phænomena which it presents.

A magnetised bar which I held in my hand appeared constantly to attract, by its north pole, the south face of the suspended needle; the south pole appeared equally to attract the

 north

north face; so that I have caused demi-revolutions to the horizontal needle, by bringing the south pole of the magnetised bar from the south face of the suspended needle, or the north pole from the north side. I could even produce an entire revolution; for the *attracted* * face follows the movement of the face which attracts it.

A tendency might also be observed in the needle to direct one of its faces towards the north, and the other towards the magnetic south; for when diverted from it, it returned by a series of oscillations, and if it had made an entire revolution, it maintained itself there; which shows that the torsion of the silk thread was too feeble to remove the needle from its position of equilibrium.

These experiments would seem to prove that, independently of magnetic poles in a longitudinal direction, the suspended needle had also poles in a diametrical direction. It is easy however to be convinced that no such poles as the latter exist.

In fact, supposing that the suspended needle were a curved plane, the magnetic action of the earth will always direct the plane of that curve in the line of the meridian, and if turned away from it, it will come back to that line. Further, the concavity will be turned towards the pole of the earth which attracts the inferior pole of the needle. If a magnetised bar is brought near which has more action than the earth on the suspended needle, the concavity or convexity of the arch will be directed according as the bar exercises an attraction or a repulsion upon the inferior pole of the needle.

This curve in the needle suffices, then, to produce the effects observed. That it is the true cause of this apparent polarization, any one may convince himself.

It will, in the first place, be readily admitted, that it is at least very difficult to make a steel wire without a slight curve, whatever care may be taken to rectify it perfectly. It will be seen, besides, by rendering this curve sensible to the eye, and presenting it successively in different directions, that it is always the curve which determines the disposition of the faces of the needle.

It may hence be concluded, that the case is the same with a curve less sensible, or even altogether invisible. The same explanation will apply to the case where the curve is complicated, and cannot be comprehended in a plane. A needle not magnetised presents analogous phænomena.

I have dwelt on these latter facts, because they include all the circumstances necessary for the detection of error, and may perhaps explain the magnetic phænomena announced by some philosophers.

* This expression is not quite exact, but is more descriptive than any other.

XL. Upon

XL. *Upon the different Qualities of the Alburnum of Spring- and Winter-felled Oak Trees.* *By* THOMAS ANDREW KNIGHT, *Esq. F.R.S.**

THE timber of oak trees felled in winter was formerly very generally believed to be much superior in quality to that afforded by similar trees felled in spring; and the same opinion appears to be still rather extensively entertained; though the practice of felling in winter has wholly ceased, on account of the increased value of the bark. But efforts have been made, and supposed to have been successful, to obtain the advantages of both seasons of felling, by taking off the bark in spring, and suffering the tree to stand till the ensuing winter. A good many facts which had come within my own observation, and information which I received from other sources, had satisfied me that the durability of the alburnum, at least, of oak trees is considerably increased by this mode of management; and I was, consequently, led to make a few experiments (with the result of which I now take the liberty to trouble the Royal Society) with the hope of discovering the cause of this supposed superiority in the quality of the wood of winter-felled trees.

In the spring of 1817, two oak trees, of nearly the same age, and growing contiguously in the same soil, were selected, each being somewhat less than a century old. The one was deprived of its bark, to as great an extent as the inexperience of my workmen permitted me to have done without danger to the tree, and it was then suffered to remain standing. The other tree was felled, and, in the usual manner, immediately stript of its bark; and the trunk was then removed to a situation in which it was securely protected from the sun and rain. The following winter, in December, the first tree (which still retained life) was felled, and its trunk immediately placed in the same situation with that of the other tree; pieces of each, selected from similar parts, have been subjected to the following experiments at different subsequent periods.

Small blocks, of similar form and size, were taken from the alburnum of each tree; and after these had ceased to lose weight, in a very warm and dry situation, the specific gravity of each was ascertained; when that of the alburnum of the spring-felled tree was found to be 0·666, and that of the same substance of the winter-felled tree to be 0·565, taking the average of several pieces of each. I had anticipated a loss of weight to about this amount in the alburnum of the winter-felled tree, having inferred, from former experiments, that it must have given out a large quantity

* From the Transactions of the Royal Society for 1820. Part II.

of

of matter in the spring and early part of the summer, to form the leaves and young shoots, which quantity could not have been restored to it during the summer, on account of the descending current of sap through the bark having been wholly intercepted.

Small blocks of equal weight of the alburnum of each tree were divided by cleaving into thin pieces; and these, after having become perfectly dry, were suspended together during ten days, in a somewhat damp room; when 1000 grains of the alburnum of the spring-felled tree were found to have gained 162 grains, and an equal weight of that of the winter-felled tree 145 grains; and I found that each substance permanently retained moisture nearly in the same proportion that it absorbed it. The alburnum of the oak, as of other trees, therefore, undergoes some change of properties in the spring; and I do not entertain any doubt but that, in all cases in which it is expedient to give durability to that substance, much advantage may be obtained by taking off the bark in spring, and suffering the trees to stand till winter. The durability of the alburnum of large oak-trees of British growth is not, however, generally an object of much consequence; because it almost always lies wholly exterior to the heart wood; but in the oak timber, which is imported from the North of Europe, the alburnum and heart wood are very often intermixed, the growth of ten or a dozen years, or more, of alburnum and heart wood composing, in alternate layers of unequal depth, the whole body of the tree; and the value of the timber of such trees is probably much affected by the season of felling.

Many experiments, similar to the preceding, were made upon the heart wood, in which I found the disposition to absorb moisture, somewhat greater in that of the spring-felled, than in that of the winter-felled tree; and I scarcely entertain any doubt but that the winter-felled heart wood is the best and most durable; but I do not think any conclusion can safely be drawn till the heart wood of many trees has been subjected to experiment; and therefore, as I have no evidence to offer which is in any degree conclusive, I shall not at present trespass further upon the attention of the Society.

Downton, March 29, 1820.

XLI. *A Table of the Sun's Declination to every Ten Minutes of his Longitude: with the Differences and Secular Variation for Jan. 1, 1801. (Obliq. of the Eclip. 23° 27′ 57″, and Sec. Var. 52″·1.) Calculated from* TAYLOR'S *Tables of Logarithms. By Mr.* JAMES UTTING, *Lynn Regis.*

[Continued from p. 186.]

Argu-

Argu-ment.	Signs 0 North. VI. South.			Signs I. North. VII. South.			Signs II. North. VIII. South.			Argu-ment.
☉'s Long.	Declination.	Diff.	Sec. Var.	Declination.	Diff.	Sec. Var.	Declination.	Diff.	Sec. Var.	☉'s Long.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
0 0	0 0 0·00	238·92	0·00	11 29 3·83	210·99	24·39	20 10 22·19	126·98	44·09	30 0
10	0 3 58·92	238·92	0·14	11 32 34·82	210·66	24·52	20 12 29·17	126·34	44·18	50
20	0 7 57·84	238·92	0·28	11 36 5·48	210·36	24·65	20 14 35·51	125·74	44·26	40
30	0 11 56·76	238·91	0·42	11 39 35·84	210·04	24·78	20 16 41·25	125·10	44·35	30
40	0 15 55·67	238·90	0·56	11 43 5·88	209·72	24·90	20 18 46·35	124·51	44·43	20
50	0 19 54·57	238·90	0·70	11 46 35·60	209·41	25·03	20 20 50·86	123·88	44·52	10
1 0	0 23 53·47	238·88	0·84	11 50 5·01	209·08	25·16	20 22 54·74	123·24	44·60	29 0
10	0 27 52·35	238·88	0·98	11 53 34·09	208·76	25·29	20 24 57·98	122·64	44·69	50
20	0 31 51·23	238·85	1·12	11 57 2·85	208·43	25·41	20 27 0·62	122·01	44·77	40
30	0 35 50·08	238·85	1·25	12 0 31·28	208·11	25·54	20 29 2·63	121·41	44·85	30
40	0 39 48·93	238·83	1·39	12 3 59·39	207·78	25·67	20 31 4·04	120·75	44·93	20
50	0 43 47·76	238·81	1·53	12 7 27·17	207·45	25·79	20 33 4·79	120·12	45·01	10
2 0	0 47 46·57	238·78	1·67	12 10 54·62	207·12	25·92	20 35 4·91	119·50	45·09	28 0
10	0 51 45·35	238·77	1·80	12 14 21·74	206·78	26·04	20 37 4·41	118·86	45·17	50
20	0 55 44·12	238·74	1·94	12 17 48·52	206·45	26·17	20 39 3·27	118·25	45·24	40
30	0 59 42·86	238·72	2·08	12 21 14·97	206·12	26·29	20 41 1·52	117·61	45·32	30
40	1 3 41·58	238·69	2·22	12 24 41·09	205·76	26·41	20 42 59·13	116·98	45·40	20
50	1 7 40·27	238·66	2·36	12 28 6·85	205·44	26·54	20 44 56·11	116·33	45·47	10
3 0	1 11 38·93	238·63	2·50	12 31 32·29	205·09	26·66	20 46 52·44	115·70	45·55	27 0
10	1 15 37·56	238·60	2·63	12 34 57·38	204·75	26·79	20 48 48·14	115·04	45·63	50
20	1 19 36·16	238·56	2·77	12 38 22·13	204·40	26·91	20 50 43·18	114·43	45·70	40
30	1 23 34·72	238·53	2·91	12 41 46·53	204·05	27·04	20 52 37·61	113·77	45·78	30
40	1 27 33·25	238·49	3·05	12 45 10·58	203·70	27·16	20 54 31·38	113·15	45·85	20
50	1 31 31·74	238·45	3·19	12 48 34·28	203·36	27·29	20 56 24·53	112·47	45·93	10
4 0	1 35 30·19	238·41	3·33	12 51 57·64	202·99	27·41	20 58 17·00	111·85	46·00	26 0
10	1 39 28·60	238·37	3·47	12 55 20·63	202·60	27·54	21 0 8·85	111·19	46·07	50
20	1 43 26·97	238·32	3·61	12 58 43·29	202·27	27·66	21 2 0·04	110·54	46·15	40
30	1 47 25·29	238·28	3·75	13 2 5·56	201·93	27·79	21 3 50·58	109·91	46·22	30
40	1 51 23·57	238·23	3·89	13 5 27·49	201·58	27·92	21 5 40·49	109·25	46·29	20
50	1 55 21·80	238·18	4·03	13 8 49·07	201·21	28·04	21 7 29·74	108·59	46·37	10
5 0	1 59 19·98	238·13	4·17	13 12 10·28	200·83	28·17	21 9 18·33	107·95	46·44	25 0
10	2 3 18·11	238·08	4·30	13 15 31·11	200·50	28·29	21 11 6·28	107·29	46·52	50
20	2 7 16·19	238·02	4·44	13 18 51·61	200·10	28·41	21 12 53·57	106·62	46·59	40
30	2 11 14·21	237·97	4·58	13 22 11·71	199·74	28·54	21 14 40·19	105·98	46·66	30
40	2 15 12·18	237·91	4·72	13 25 31·45	199·38	28·66	21 16 26·17	105·31	46·73	20
50	2 19 10·09	237·85	4·86	13 28 50·83	199·00	28·78	21 18 11·48	104·67	46·80	10
6 0	2 23 7·94	237·78	5·00	13 32 9·83	198·63	28·90	21 19 56·15	103·98	46·87	24 0
10	2 27 5·72	237·73	5·13	13 35 28·46	198·24	29·02	21 21 40·13	103·33	46·94	50
20	2 31 3·45	237·66	5·27	13 38 46·70	197·88	29·15	21 23 23·46	102·67	47·01	40
30	2 35 1·11	237·59	5·41	13 42 4·58	197·49	29·27	21 25 6·13	102·00	47·08	30
40	2 38 58·70	237·53	5·55	13 45 22·07	197·12	29·39	21 26 48·13	101·35	47·15	20
50	2 42 56·23	237·45	5·69	13 48 39·19	196·73	29·52	21 28 29·48	100·67	47·22	10
7 0	2 46 53·68	237·39	5·83	13 51 55·92	196·31	29·64	21 30 10·15	100·02	47·28	23 0
10	2 50 51·07	237·31	5·96	13 55 12·26	195·96	29·76	21 31 50·17	99·32	47·35	50
20	2 54 48·38	237·23	6·10	13 58 28·22	195·57	29·88	21 33 29·49	98·66	47·41	40
30	2 58 45·61	237·17	6·24	14 1 43·79	195·19	30·00	21 35 8·15	98·00	47·48	30
40	3 2 42·77	237·08	6·38	14 4 58·98	194·79	30·11	21 36 46·15	97·30	47·55	20
50	3 6 39·85	237·00	6·52	14 8 13·77	194·39	30·23	21 38 23·45	96·66	47·61	10
	Signs V. N. XI. S.			Signs IV. N. X. S.			Signs III. N. IX. S.			

TABLE continued.

Argument ☉'s Long. °	′	Signs 0 North. VI. South. Declination. ° ′ ″	Diff. ″	Sec. Var. ″	Signs I. North. VII. South. Declination. ° ′ ″	Diff. ″	Sec. Var. ″	Signs II. North. VIII. South. Declination. ° ′ ″	Diff. ″	Sec. Var. ″	Argument ☉'s Long. °	′
8	0	3 10 36·85	236·92	6·66	14 11 28·16	194·00	30·35	21 40 0·11	95·95	47·68	22	0
	10	3 14 33·77	236·84	6·80	14 14 42·16	193·60	30·47	21 41 36·06	95·30	47·75		50
	20	3 18 30·61	236·75	6·93	14 17 55·76	193·22	30·59	21 43 11·36	94·62	47·81		40
	30	3 22 27·36	236·66	7·07	14 21 8·98	192·79	30·71	21 44 45·98	93·93	47·88		30
	40	3 26 24·02	236·58	7·21	14 24 21·77	192·40	30·82	21 46 19·91	93·26	47·94		20
	50	3 30 20·60	236·48	7·35	14 27 34·17	192·00	30·94	21 47 53·17	92·56	48·01		10
9	0	3 34 17·08	236·40	7·49	14 30 46·[illegible]	191·60	31·06	21 49 25·73	91·90	48·07	21	0
	10	3 38 13·48	236·29	7·63	14 33 57·77	191·19	31·18	21 50 57·63	91·20	48·14		50
	20	3 42 9·77	236·21	7·77	14 37 8·96	190·77	31·30	21 52 28·83	90·52	48·20		40
	30	3 46 5·98	236·10	7·91	14 40 19·73	190·37	31·42	21 53 59·35	89·84	48·26		30
	40	3 50 2·08	236·01	8·05	14 43 30·10	189·95	31·54	21 55 29·19	89·15	48·32		20
	50	3 53 58·09	235·91	8·19	14 46 40·05	189·54	31·66	21 56 58·34	88·47	48·38		10
10	0	3 57 54·00	235·80	8·32	14 49 49·59	189·12	31·78	21 58 26·81	87·76	48·44	20	0
	10	4 1 49·80	235·70	8·46	14 52 58·71	188·71	31·90	21 59 54·57	87·08	48·50		50
	20	4 5 45·50	235·60	8·60	14 56 7·42	188·29	32·02	22 1 21·65	86·41	48·56		40
	30	4 9 41·10	235·49	8·74	14 59 15·71	187·85	32·14	22 2 48·06	85·67	48·62		30
	40	4 13 36·59	235·37	8·87	15 2 23·56	187·45	32·25	22 4 13·73	85·02	48·67		20
	50	4 17 31·96	235·27	9·01	15 5 31·01	187·02	32·37	22 5 38·75	84·33	48·73		10
11	0	4 21 27·23	235·16	9·15	15 8 38·03	186·58	32·49	22 7 3·08	83·59	48·79	19	0
	10	4 25 22·39	235·04	9·29	15 11 44·61	186·17	32·61	22 8 26·67	82·93	48·85		50
	20	4 29 17·43	234·92	9·42	15 11 50·78	185·74	32·72	22 9 49·60	82·23	48·90		40
	30	4 33 12·35	234·81	9·56	15 17 56·52	185·29	32·84	22 11 11·83	81·50	48·96		30
	40	4 37 7·16	234·69	9·70	15 21 1·81	184·85	32·96	22 12 33·33	80·84	49·01		20
	50	4 41 1·85	234·56	9·83	15 24 6·66	184·45	33·07	22 13 54·17	80·12	49·07		10
12	0	4 44 56·41	234·45	9·97	15 27 11·11	183·98	33·19	22 15 14·29	79·44	49·12	18	0
	10	4 48 50·86	234·32	10·11	15 30 15·09	183·55	33·30	22 16 33·73	78·71	49·17		50
	20	4 52 45·18	234·19	10·24	15 33 18·64	183·12	33·42	22 17 52·44	78·01	49·22		40
	30	4 56 39·37	234·06	10·38	15 36 21·76	182·68	33·53	22 19 10·45	77·31	49·27		30
	40	5 0 33·43	233·94	10·52	15 39 24·44	182·23	33·64	22 20 27·76	76·61	49·32		20
	50	5 4 27·37	233·80	10·65	15 42 26·67	181·78	33·76	22 21 44·37	75·88	49·37		10
13	0	5 8 21·17	233·67	10·79	15 45 28·45	181·33	33·87	22 23 0·25	75·20	49·42	17	0
	10	5 12 14·84	233·54	10·93	15 48 29·78	180·90	33·98	22 24 15·45	74·49	49·47		50
	20	5 16 8·38	233·40	11·07	15 51 30·68	180·43	34·10	22 25 29·94	73·79	49·52		40
	30	5 20 1·78	233·26	11·21	15 54 31·11	179·98	34·21	22 26 43·73	73·05	49·57		30
	40	5 23 55·04	233·12	11·34	15 57 31·09	179·54	34·32	22 27 56·78	72·36	49·62		20
	50	5 27 48·16	232·98	11·48	16 0 30·63	179·07	34·44	22 29 9·14	71·62	49·67		10
[illegible]	0	5 31 41·14	232·85	[illegible]	16 3 29·70	178·62	34·55	22 30 20·76	70·95	49·72	16	0
	10	5 35 33·99	232·67	11·76	16 6 28·32	178·15	34·66	22 31 31·71	70·21	49·77		50
	20	5 39 26·66	232·54	11·89	16 9 26·47	177·68	34·77	22 32 41·92	69·49	49·82		40
	30	5 43 19·20	232·40	12·03	16 12 24·15	177·24	34·89	22 33 51·41	68·79	49·86		30
	40	5 47 11·60	232·25	12·17	16 15 21·39	176·76	35·00	22 35 0·20	68·06	49·91		20
	50	5 51 3·85	232·09	12·30	16 18 18·15	176·31	35·11	22 36 8·26	67·36	49·96		10
15	0	5 54 55·94	231·94	12·44	16 21 14·46	175·82	35·22	22 37 15·62	66·62	50·01	15	0
	10	5 58 47·88	231·78	12·58	16 24 10·28	175·38	35·33	22 38 22·24	65·92	50·06		50
	20	6 2 39·66	231·63	12·71	16 27 5·66	174·89	35·44	22 39 28·16	65·19	50·10		40
	30	6 6 31·29	231·46	12·85	16 30 0·55	174·41	35·55	22 40 33·35	64·47	50·15		30
	40	6 10 22·75	231·31	12·98	16 32 54·96	173·94	35·66	22 41 37·82	63·76	50·19		20
	50	6 14 14·06	231·14	13·12	16 35 48·90	173·47	35·77	22 42 41·58	63·02	50·24		10
		Signs V. N. XI. S.			Signs IV. N. X. S.			Signs III. N. IX. S.				

TABLE continued.

Argument ☉'s Long.	Signs 0 North. VI. South. Declination.	Diff.	Sec. Var.	Signs I. North. VII. South. Declination.	Diff.	Sec. Var.	Signs II. North. VIII. South. Declination.	Diff.	Sec. Var.	Argument ☉'s Long.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
16 0	6 18 5·20	230·98	13·25	16 38 42·37	172·99	35·88	22 43 44·60	62·30	50·28	14 0
10	6 21 56·18	230·81	13·39	16 41 35·36	172·51	35·99	22 44 46·90	61·60	50·32	50
20	6 25 46·99	230·65	13·52	16 44 27·87	172·02	36·10	22 45 48·50	60·84	50·37	40
30	6 29 37·64	230·48	13·66	16 47 19·89	171·55	36·21	22 46 49·34	60·14	50·41	30
40	6 33 28·12	230·31	13·80	16 50 11·44	171·05	36·32	22 47 49·48	59·40	50·45	20
50	6 37 18·43		13·93	16 53 2·49		36·43	22 48 48·88		50·50	10
		230·13			170·58			58·68		
17 0	6 41 8·56	229·96	14·07	16 55 53·07	170·07	36·54	22 49 47·56	57·96	50·54	13 0
10	6 44 58·52	229·78	14·21	16 58 43·14	169·60	36·65	22 50 45·52	57·20	50·58	50
20	6 48 48·30	229·60	14·34	17 1 32·74	169·10	36·76	22 51 42·72	56·50	50·61	40
30	6 52 37·90	229·43	14·48	17 4 21·84	168·60	36·87	22 52 39·22	55·78	50·65	30
40	6 56 27·33	229·24	14·61	17 7 10·44	168·10	36·97	22 53 35·00	55·00	50·69	20
50	7 0 16·57		14·75	17 9 58·54		37·08	22 54 30·00		50·72	10
		229·06			167·64			54·32		
18 0	7 4 5·63	228·88	14·88	17 12 46·18	167·10	37·19	22 55 21·32	53·56	50·76	12 0
10	7 7 54·51	228·68	15·02	17 15 33·28	166·63	37·30	22 56 17·88	52·84	50·79	50
20	7 11 43·19	228·50	15·15	17 18 19·91	166·10	37·40	22 57 10·72	52·10	50·83	40
30	7 15 31·69	228·31	15·29	17 21 6·01	165·62	57·51	22 58 2·82	51·36	50·86	30
40	7 19 20·00	228·12	15·42	17 23 51·63	165·10	37·61	22 58 54·18	50·64	50·89	20
50	7 23 8·12		15·56	17 26 36·73		37·72	22 59 41·32		50·93	10
		227·92			164·60			49·90		
19 0	7 26 56·04	227·74	15·69	17 29 21·33	164·07	37·82	23 0 31·72	49·16	50·96	11 0
10	7 30 43·78	227·53	15·83	17 32 5·40	163·60	37·93	23 1 23·88	48·43	50·99	50
20	7 34 31·31	227·33	15·96	17 34 49·00	163·06	38·03	23 2 12·31	47·67	51·03	40
30	7 38 18·64	227·14	16·10	17 37 32·06	162·55	38·14	23 2 59·98	46·96	51·06	30
40	7 42 5·78	226·93	16·23	17 40 14·61	162·04	38·24	23 3 46·94	46·20	51·09	20
50	7 45 52·71		16·37	17 42 56·65		38·35	23 4 33·14		51·13	10
		226·72			161·53			45·48		
20 0	7 49 39·43	226·52	16·50	17 45 38·18	161·00	38·45	23 5 18·62	44·73	51·16	10 0
10	7 53 25·95	226·32	16·63	17 48 19·18	160·48	38·55	23 6 3·35	43·99	51·20	50
20	7 57 12·27	226·10	16·77	17 50 59·66	159·97	38·65	23 6 47·31	43·25	51·23	40
30	8 0 58·37	225·90	16·90	17 53 39·63	159·43	38·76	23 7 30·59	42·51	51·26	30
40	8 4 44·27	225·68	17·03	17 56 19·06	158·89	38·86	23 8 13·10	41·73	51·29	20
50	8 8 29·95		17·17	17 58 57·95		38·96	23 8 54·88		51·32	10
		225·46			158·39			41·00		
21 0	8 12 15·41	225·25	17·30	18 1 36·34	157·86	39·06	23 9 35·88	40·28	51·35	9 0
10	8 16 0·66	225·04	17·44	18 4 14·20	157·33	39·16	23 10 16·16	39·55	51·38	50
20	8 19 45·70	224·80	17·57	18 6 51·53	156·80	39·26	23 10 55·71	38·79	51·40	40
30	8 23 30·50	224·60	17·71	18 9 28·33	156·25	39·37	23 11 34·50	38·05	51·43	30
40	8 27 15·10	224·37	17·84	18 12 4·58	155·73	39·47	23 12 12·55	37·31	51·46	20
50	8 30 59·47		17·98	18 14 40·31		39·57	23 12 49·86		51·48	10
		224·14			155·17			36·55		
22 0	8 34 43·61	223·92	18·11	18 17 15·48	154·66	39·67	23 [illegible] 26·41	35·79	51·51	8 0
10	8 38 27·53	223·69	18·24	18 19 50·14	154·11	39·77	23 14 2·20	35·09	51·53	50
20	8 42 11·22	223·46	18·37	18 22 24·25	153·58	39·86	23 14 37·29	34·30	51·55	40
30	8 45 54·68	223·22	18·51	18 24 57·83	153·01	39·96	23 15 11·59	33·57	51·57	30
40	8 49 37·90	222·99	18·64	18 27 30·84	152·49	40·06	23 15 45·16	32·82	51·59	20
50	8 53 20·89		18·77	18 30 3·33		40·15	23 16 17·98		51·61	10
		222·76			151·92			32·06		
23 0	8 57 3·65	222·52	18·90	18 32 35·25	151·38	40·25	23 16 50·04	31·35	51·63	7 0
10	9 0 46·17	222·29	19·03	18 35 6·63	150·85	40·35	23 17 21·39	30·57	51·65	50
20	9 4 28·46	222·04	19·17	18 37 37·48	150·28	40·45	23 17 51·96	29·82	51·67	40
30	9 8 10·50	221·79	19·30	18 40 7·76	149·74	40·55	23 18 21·78	29·08	51·69	30
40	9 11 52·29	221·57	19·43	18 42 37·50	149·18	40·65	23 18 50·86	28·30	51·71	20
50	9 15 33·86		19·57	18 45 6·68		40·75	23 19 19·16		51·73	10
		221·30			148·61			27·57		
	Signs V. N. XI. S.			Signs IV. N. X. S.			Signs III. N. IX. S.			

TABLE continued.

Argument.	Signs 0 North. VI. South.			Signs I. North. VII. South.			Signs II. North. VIII. South.			Argument.
⊙'s Long.	Declination.	Diff.	Sec. Var.	Declination.	Diff.	Sec. Var.	Declination.	Diff.	Sec. Var.	⊙'s Long.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
24 0	9 19 15·16	221·06	19·70	18 47 35·29	148·06	40·85	23 19 46·73	26·84	51·75	6 0
10	9 22 56·22	220·81	19·83	18 50 3·35	147·50	40·95	23 20 13·57	26·06	51·77	50
20	9 26 37·03	220·57	19·96	18 52 30·85	146·95	41·04	23 20 39·63	25·33	51·79	40
30	9 30 17·60	220·30	20·09	18 54 57·80	146·39	41·14	23 21 4·96	24·57	51·81	30
40	9 33 57·90	220·05	20·22	18 57 24·19	145·81	41·23	23 21 29·53	23·80	51·83	20
50	9 37 37·95	219·80	20·35	18 59 50·00	145·26	41·33	23 21 53·33	23·06	51·85	10
25 0	9 41 17·75	219·54	20·48	19 2 1[illegible]	144·69	41·42	23 22 16·39	22·32	51·86	5 0
10	9 44 57·29	219·29	20·61	19 4 39·95	144·12	41·51	23 22 38·71	21·56	51·88	50
20	9 48 36·58	219·02	20·75	19 7 [illegible]	143·53	41·60	23 23 0·27	20·77	51·89	40
30	9 52 15·60	218·75	20·88	19 9 27·60	142·98	41·70	23 23 21·04	20·06	51·91	30
40	9 55 54·35	218·49	21·01	19 11 50·58	142·40	41·79	23 23 41·10	19·30	51·93	20
50	9 59 32·84	218·23	21·15	19 14 12·98	141·82	41·88	23 24 0·40	18·54	51·94	10
26 0	10 3 11·07	217·95	21·28	19 16 34·80	141·27	41·97	23 24 18·94	17·79	51·96	4 0
10	10 6 49·02	217·69	21·41	19 18 56·07	140·66	42·06	23 24 36·73	17·03	51·97	50
20	10 10 26·71	217·40	21·54	19 21 16·73	140·09	42·16	23 24 53·76	16·26	51·98	40
30	10 14 4·11	217·15	21·67	19 23 36·82	139·51	42·25	23 25 10·02	15·51	51·99	30
40	10 17 41·26	216·85	21·80	19 25 56·33	138·93	42·34	23 25 25·53	14·78	52·00	20
50	10 21 18·11	216·59	21·93	19 28 15·26	138·35	42·44	23 25 40·31	14·00	52·01	10
27 0	10 24 54·70	216·31	22·06	19 30 33·61	137·75	42·53	23 25 54·31	13·25	52·02	3 0
10	10 28 31·01	216·03	22·19	19 32 51·36	137·18	42·62	23 26 7·56	12·50	52·03	50
20	10 32 7·04	215·74	22·32	19 35 8·54	136·58	42·71	23 26 20·06	11·74	52·04	40
30	10 35 42·78	215·46	22·45	19 37 25·12	135·98	42·80	23 26 31·80	10·98	52·04	30
40	10 39 18·24	215·17	22·58	19 39 41·10	135·41	42·89	23 26 42·78	10·22	52·05	20
50	10 42 53·41	214·89	22·71	19 41 56·51	134·81	42·98	23 26 53·00	9·47	52·05	10
28 0	10 46 28·30	214·59	22·84	19 44 11·32	134·21	43·07	23 27 2·47	8·69	52·06	2 0
10	10 50 2·89	214·31	22·97	19 46 25·53	133·62	43·16	23 27 11·16	7·96	52·06	50
20	10 53 37·20	214·01	23·10	19 48 39·15	133·02	43·24	23 27 19·12	7·21	52·06	40
30	10 57 11·21	213·72	23·23	19 50 52·17	132·42	43·33	23 27 26·33	6·43	52·06	30
40	11 0 44·93	213·41	23·36	19 53 4·59	131·81	43·42	23 27 32·76	5·69	52·07	20
50	11 4 18·34	213·13	23·49	19 55 16·40	131·22	43·50	23 27 38·45	4·90	52·07	10
29 0	11 7 51·47	212·81	23·62	19 57 27·62	130·62	43·59	23 27 43·35	4·19	52·08	1 0
10	11 11 24·28	212·52	23·75	19 59 38·24	130·02	43·67	23 27 47·54	3·40	52·08	50
20	11 14 56·80	212·22	23·88	20 1 48·26	129·38	43·76	23 27 50·94	2·66	52·08	40
30	11 18 29·02	211·91	24·01	20 3 57·64	128·81	43·84	23 27 53·60	1·90	52·09	30
40	11 22 0·93	211·61	24·13	20 6 6·45	128·18	43·92	23 27 55·50	1·13	52·09	20
50	11 25 32·54	211·29	24·26	20 8 14·63	127·56	44·01	23 27 56·63	0·37	52·09	10
30 0	11 29 3·83		24·39	20 10 22·19		44·09	23 27 57·00		52·10	0 0
	Signs V. North. XI. South.			Signs IV. North. X. South.			Signs III. North. IX. South.			

Enter the Table with the ⊙'s longitude as the argument, and take out the corresponding declination, adding the proportional part for the odd minutes, to which apply the secular variation: the result is the ⊙'s declination as required.

N. B. The secular variation is to be subtracted from the ⊙'s declination as given by the Table, if the time is subsequent to 1801; otherwise it is to be added thereto.

XLII. *Dis-*

XLII. *Discovery of Chromate of Iron in Shetland.* By Samuel Hibbert, *M.D. Edinburgh**.

5, Hill Place, Edinburgh, Feb. 7, 1820.

Sir,—I have the honour to make a communication to the Society of Arts, &c., respecting the discovery, which I originally made two years and a half ago, of the chromate of iron in the Shetland Islands, which substance is at present obtained for the manufacturers of colours at a considerable expense from the United States of America. Since a notice first appeared in the journals relating to the discovery, considerable inquiries have been made concerning it; but, that I might not create expectations which could not be realised, I was unwilling to make further communications on the subject until I had made a second visit to Shetland, when I ascertained that it exists there in great abundance. Conceiving, therefore, that the patriotic Institution of London for the promotion of the arts and commerce of Great Britain was the most suitable medium through which the knowledge of the place and circumstances under which the chromate of iron is found might be first communicated to those who are commercially interested in the discovery, I have taken the liberty of transmitting to you a set of specimens †, which, in reference to the annexed description, will illustrate the varied character of the mineral. If my discovery shall be considered as a contribution to the commercial resources of the British Islands, it will afford me some recompense for the considerable time and labour which I have expended in the prosecution of the search after this important ore. I am, sir, &c.

A. Aikin, Esq. Sec. &c. Samuel Hibbert, M.D.

Circumstances under which the Chromate of Iron is found in Shetland.

The chromate of iron occurs in the Serpentine rocks in the neighbourhood of Balta Sound, in the island of Unst. I was first led to a search after this ore by observing innumerable fragments of it strewed about the hill in which it is found, and even contributing to strengthen the fences of the country. It is observed in the form of imbedded and insulated masses, at Buness, close to the house of the proprietor, Thomas Edmonstone, esq. The extent of the greatest mass is not, however, ascertained, as it is on one side concealed by the sea, and on the other by the deep soil of a meadow. It was traced three feet in breadth and fifteen feet in length. At Hagdale, near Haroldwick, the chromate of iron occurs in the form of numerous thin ramifying veins;

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1820. The Isis gold medal of the Society was voted to Dr. Hibbert for this communication.

† Which are now in the Repository of the Society.

 but

but these are only from two to three inches in breadth, sometimes increasing to the breadth of five or six inches. Many masses are elsewhere observable, extending a few feet and then losing themselves in a general dissemination throughout the serpentine rock in which they occur. This dissemination consists in the diffusion of granular particles of the colour and size of gunpowder.

It is evident from this description that the most promising appearance of the ore is adjoining the house of Mr. Edmonstone; and from this gentleman, whom I have made acquainted with all the circumstances of the mineral, any commercial inquiry will meet with the most satisfacto[illegible]wer. Letters may be directed, "Thomas Edmonstone, [illegible] of Buness, Island of Unst, Shetland."

Upon the encourag[illegible]owever, from London will depend the renewed searches a[illegible] the chromate of iron, not only on Mr. Edmonstone's grounds, but in the adjoining hills, which are the joint property of several landed proprietors in Unst. From the quantity, therefore, of the ore which has been found in detached portions on the hills, and from the promising appearance on Mr. Edmonstone's grounds, I would submit to the manufacturing chemists in London the propriety of rendering to the Shetland gentlemen every scientific assistance which they may require from their advice, or even, if wanted, from other exertions in prosecuting the search after this ore, provided its quality suits their purpose. It appears to me that some serious obstacles cannot fail to result from the inexperience of the Shetland gentlemen in whatever concerns the operations of mining.

Vessels trading from Leith to Shetland visit Balta Sound almost every month in the course of the spring and summer.

In furnishing the foregoing particulars relating to the chromate of iron, I have only to add that I shall be happy to answer any personal inquiries on the subject, concerning whatever I may have left unexplained. SAMUEL HIBBERT, M.D.

Portions of the specimens of chromate of iron transmitted by Dr. Hibbert were put into the hands of several members of the Society for examination, with respect both to the quality and richness of the ore as compared with that imported from the United States. Samples of chromate of lead prepared from the American and Shetland varieties of chromate of iron were laid before the Committee by Mr. Midgley, one of the chairmen of the committee of chemistry, who has had large experience in this branch of chemical manufacture. The result of this gentleman's investigation (confirmed by the experiments made by other members of the Society) is, that the ore from Shetland in quality is quite equal to that imported from America, and in richness, as far as can be judged from a few specimens, is superior.

XLIII. *Ob-*

XLIII. *Observations relating to the Depression of Mercury in Glass Tubes; occasioned by an Article in the last Quarterly Journal of Science.* By JAMES IVORY, *M.A. F.R.S.*

To Mr. Tilloch.

SIR, — I HAVE to beg the favour of your inserting in your next publication, the following observations occasioned by an article in the last Quarterly Journal of Science. They relate to the depression of mercury in glass tubes, for computing which two tables have been published in the Supplement of the *Encyclopædia Britannica;* one under the head of COHESION OF FLUIDS, and the other under that of FLUIDS.

It is necessary to begin with saying a few words of the *series* by which the first of the two tables is constructed. If we neglect all the terms of each of the coefficient-serieses, except the first, as may be done in capillary tubes with very minute bores, the series for computing the depression will become

$$s = (bx) + \frac{qx^2}{48} \cdot (bx)^3 + \frac{qx^2}{128} (bx)^5 + \frac{qx^2}{256} (bx)^7 + \&c.$$

Now here, all the coefficients after the first being small, the product bx, which I consider as the quantity sought, will not be much different from s; and it is manifest that, when s is considerable, a great number of the terms must be taken in, if the approximation is to be carried to many figures, the coefficients decreasing slowly. In the case of glass and mercury, s is nearly $\frac{3}{4}$; and, taking into account the rate of decrease of the coefficients, the total convergency of the series may be reckoned at $\frac{1}{4}$; and the first four or five terms will give an approximation extending to five figures. Accordingly it will be found that, in the table, the depressions for the smallest bores are nearly exact.

But the case is different when the diameters of the tubes have a greater magnitude. Then the values of the coefficient-serieses increase greatly, and cannot be computed with any degree of certainty from two or three of their first terms. In reality all these serieses, excepting the first which is very regular and convergent, diverge in their first terms instead of converging, when $\frac{qx^2}{4}$ is nearly equal to, or greater than, unit. The divergency indeed goes on to a certain point only, after which the serieses will converge, and they will ultimately converge very rapidly as in the first one. In this manner of computing therefore, when the tubes have large bores, the degree of exactness in the result will depend entirely on the three first terms of the value of the sine of depression; since these terms are the only ones in which the co-

efficients of the unknown quantity can be computed with any tolerable precision.

If the exactness of the result would be affected in the fourth and fifth decimal places by omitting entirely all the terms in the value of the sine except the three first, it is to be feared that a like inaccuracy will occur although some of the omitted terms be taken in, but with inexact coefficients, greatly short of the real values, not amounting perhaps to the 10th, or the 100dth, or even the 1000dth, part of the truth.

Such appears to be the objection to this mode of computation; and the coefficient-serie[illegible]r the first and second, are so complicated that the disadv[illegible]does not appear to admit of a remedy. Still however th[illegible]d is possessed of considerable accuracy, which it owes t[illegible]tance, noticed by the author, that the first term alone brings out the truth within a $\frac{1}{30}$th of the whole.

The rules by which the other table is constructed are investigated on this principle; that the quantity denoted by f is very nearly equal to unit in the case of small capillary tubes, and even in the largest bores it decreases only to a certain limit which is greater than $\frac{2}{3}\frac{4}{5}$. A formula is therefore sought for determining f when it differs sensibly from the limit; and in all other cases, the same quantity is supposed to coincide with the limit. The rules in the article Fluids in the Supplement to the *Encyclopædia* will determine the depression to five places of figures, and are therefore more than sufficient for any practical purpose. But it is by no means supposed that it is impossible to find other and better rules for the same purpose; as I shall presently show by giving another formula, which has the advantage of determining the depression directly from the given quantities without the solution of an equation. I put q for the depression sought; l, for the diameter of the tube; z, for the sine of depression $= \cdot 735$; $t = \left(\frac{7l}{2}\right)^2$; and

$$\lambda = 1 + \frac{1}{1^2}\cdot\frac{t}{2} + \frac{1}{1^2.2^2}\cdot\frac{t^2}{3} + \frac{1}{1^2.2^2.3^2}\cdot\frac{t^3}{4} + \&c.;\ \text{then}$$

$$\begin{aligned} q = \frac{z}{49.l.\lambda} &- \frac{l}{4}\cdot\frac{z^3}{\lambda^3}\cdot\left\{\frac{1}{12} + \frac{t}{36} + \frac{11.t^2}{1440} + \frac{t^3}{1200},\right. \\ &- \frac{l}{4}\cdot\frac{z^5}{\lambda^5}\cdot\left\{\frac{1}{32} + \frac{7.t}{384} + \frac{19.t^2}{1920} + \frac{523.t^3}{161280},\right. \\ &- \frac{l}{4}\cdot\frac{z^7}{\lambda^7}\cdot\left\{\frac{1}{64} + \frac{29.t}{1920} + \frac{419.t^2}{32256},\right. \\ &- \frac{l}{4}\cdot\frac{z^9}{\lambda^9}\cdot\left\{\frac{7}{768} + \frac{41.t}{3072} + \frac{41029.t^2}{2580186}, \&c.\right. \end{aligned}$$

This formula will extend to all tubes not much exceeding six-tenths of an inch; and by means of it the results in the table may

may be confirmed. If two independent methods of computation agree in bringing out the same numerical values, a greater proof of exactness can seldom be obtained. Now having computed the depressions in the table by the formula just set down, I have found the same results as before, with slight differences in the last place of figures, except in the cases of the bores 0·5 and 0·6, in each of which a numerical error has been detected. In the first instance, $l = \cdot 5$; $t = 3\cdot 0625$; $\lambda = 3\cdot 546$; then

$$q = \frac{\cdot 01445}{l.\lambda} = \cdot 00815.$$

In the other instance, $l = 0\cdot 6$; $t = 4\cdot 41$; $\lambda = 5\cdot 579$; and

$$q = \frac{\cdot 01445}{l.\lambda} = \cdot 00431.$$

These results are confirmed by the new formula. The numbers in the table, viz. ·00835 and ·00443 are therefore both erroneous; although the real state of the case is not on this account much altered.

I may observe here that, in the long run, or for tubes large enough, the expression,

$$q = \frac{\cdot 01445}{l.\lambda},$$

will coincide with the formula given long ago by Laplace at p. 65 of the Supplement to his Theory of Capillary Action. The truth is, that λ continually tends to a final expression as the bore of the tube becomes greater; and it is only when that quantity has attained its ultimate value that the two rules will coincide. Laplace's formula fails in giving an approximation near enough the truth for tubes contained in the table; but it would be found to have all the exactness to be wished for in larger tubes, lying beyond the scope of the table.

In conclusion, sir, I am sorry that in this investigation I was obliged to introduce such obnoxious quantities as exponentials, &c., and I must express my regret that I could not accomplish what I had in view by means of the *Taylorian Theorem*, that universal solvent of all analytical difficulties, as it has lately been discovered to be. Believe me to be, &c.

April 7, 1821. J. Ivory.

XLIV. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. Grooby.

[Continued from p. 199.]

1821.

1821.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 14	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
June	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	2·97	6·88	56·49	40·10	29·36	56·66	59·66	29·61	15·67	10·93	56·52	22·14	48·56	51·59	57·63	24·49	48·98	32·88
2	3·00	91	51	11	37	67	67	61	67	93	52	14	56	49	62	48	97	88
3	03	93	53	13	38	68	68	62	67	93	52	14	55	48	61	47	97	87
4	06	96	55	14	39	69	69	62	67	93	52	14	54	47	60	47	96	87
5	00	99	58	16	41	70	71	63	67	93	51	13	53	46	59	46	96	87
6	13	70·2	60	18	42	71	72	64	67	93	51	13	52	45	58	45	95	86
7	16	05	62	19	43	72	73	65	67	93	51	13	52	44	57	44	95	86
8	19	08	65	21	45	73	74	65	67	92	50	12	51	43	56	43	94	86
9	22	11	67	23	46	74	75	66	67	92	50	12	50	42	55	42	94	85
10	25	14	69	25	48	75	76	67	67	92	50	12	49	41	54	41	93	85
11	28	17	72	27	50	77	78	68	68	92	50	12	[illegible]	40	53	41	92	85
12	32	20	74	28	51	78	79	69	68	93	50	12	[illegible]	[illegible]	52	40	91	84
13	35	23	77	30	53	79	81	70	69	93	50	12	[illegible]	[illegible]	51	39	91	83
14	38	27	79	32	55	80	82	71	69	93	50	12	47	39	50	38	90	83
15	41	30	81	34	57	82	84	73	69	94	51	13	47	38	49	37	89	83
16	44	33	84	36	59	83	85	74	70	94	51	13	46	37	48	36	88	82
17	48	36	86	38	61	84	87	75	70	95	51	13	45	37	47	35	88	81
18	51	39	89	40	63	86	89	76	71	95	51	13	45	36	46	34	87	80
19	54	43	91	42	65	87	91	77	71	95	51	14	44	35	45	33	86	80
20	57	46	94	44	67	89	93	78	72	95	51	14	44	34	44	32	85	79
21	60	49	96	46	70	91	95	80	72	96	52	15	43	34	43	31	85	78
22	64	52	99	49	72	93	97	81	73	96	52	15	43	33	42	30	84	77
23	67	55	57·01	51	75	94	99	83	74	97	53	16	43	33	41	29	84	76
24	70	58	04	53	77	96	01	84	75	97	53	16	42	32	40	28	83	75
25	74	62	07	55	80	98	15·0·03	85	76	98	53	16	42	32	39	27	82	75
26	77	65	10	57	82	99	05	87	77	99	54	17	42	31	38	26	81	74
27	80	68	13	60	85	57·01	07	88	78	11·00	54	17	41	31	37	25	81	73
28	84	71	16	62	88	03	09	90	79	00	55	18	41	30	36	24	80	72
29	87	75	18	65	90	05	11	92	80	01	56	19	40	30	36	23	79	71
30	90	78	21	67	93	07	13	94	81	02	57	20	40	30	35	22	79	70

1821.	1α Libræ	2α Libræ	ι Cor. Bor.	α Serpentis	Antares.	ι Herculis.	ι Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
June	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	51·12	2·56	9·81	30·48	29·28	32·41	40·76	55·46	47·59	5·55	33·85	45·96	9·71	21·86	37·31	46·44	52·63	10·32
2	12	56	81	48	29	42	77	48	61	57	87	99	74	89	34	47	66	35
3	13	57	81	48	30	43	79	50	64	60	90	46·02	77	92	37	51	70	39
4	13	57	81	48	31	44	80	52	66	62	92	05	80	95	40	54	72	42
5	13	57	82	49	32	46	82	54	69	65	94	08	83	99	44	58	76	46
6	13	57	82	49	33	47	83	55	71	67	97	11	86	22·02	47	61	79	49
7	13	57	82	49	34	48	84	57	74	70	99	14	89	05	50	65	82	52
8	13	57	82	50	35	49	86	59	76	72	34·02	16	91	09	53	68	86	56
9	13	57	82	50	36	50	87	61	79	75	04	19	94	12	56	72	89	59
10	12	56	82	51	36	51	88	62	81	77	07	21	96	15	59	75	92	62
11	12	56	81	51	37	52	89	64	83	79	09	24	99	18	62	79	95	66
12	12	56	81	51	37	53	90	65	85	82	12	[illegible]	0·01	20	65	82	99	69
13	12	56	81	51	38	54	91	67	87	84	14	29	04	23	68	86	53·02	73
14	12	56	81	52	38	55	92	68	89	86	16	31	[illegible]	26	70	89	05	76
15	11	56	80	52	39	56	93	69	92	88	18	34	[illegible]	29	73	93	09	80
16	11	55	80	52	39	57	95	71	94	90	20	36	[illegible]	32	76	96	12	83
17	11	55	79	52	40	58	96	72	96	93	23	39	14	34	79	47·00	16	87
18	11	55	79	53	40	59	97	74	98	95	25	41	16	37	82	03	19	91
19	11	55	78	53	41	59	98	75	48·00	97	27	44	19	40	85	07	22	94
20	11	55	78	53	41	60	99	76	02	99	29	47	22	42	88	11	25	97
21	10	54	77	53	42	60	41·00	77	04	6·01	31	49	24	45	91	14	28	11·01
22	10	54	77	53	42	61	01	79	06	03	33	52	27	47	94	18	31	05
23	10	54	76	53	43	61	01	80	08	05	35	54	29	50	97	21	34	08
24	10	54	76	53	43	62	02	81	10	07	37	56	31	52	38·00	25	37	11
25	09	53	75	52	44	62	03	82	12	09	39	58	33	55	03	28	40	15
26	09	53	75	52	44	63	03	83	14	11	41	60	35	58	06	32	43	18
27	08	52	74	52	45	63	04	84	16	13	42	63	38	60	10	35	46	21
28	08	52	74	51	46	64	04	85	18	15	45	65	40	63	12	39	49	25
29	07	51	73	51	47	64	05	86	20	17	47	67	42	65	15	42	52	28
30	06	50	73	51	47	64	05	86	21	18	48	69	44	67	17	45	55	31

XLV. *Description of an improved Glaze for Porcelain.* By Mr. JOHN ROSE, *of Coalport, Shropshire**.

Coalport, March 24, 1820.

SIR,— HAVING for some time made use of a glaze for porcelain, which gives me great satisfaction, and into the composition of which neither lead nor arsenic is admitted, I beg leave to submit the same to the consideration of the Society of Arts, &c.

The common glaze for porcelain and the finer kinds of earthenware contains a considerable proportion of glass of lead; this ingredient however, on account of its being mixed with a certain proportion of siliceous earth and other vitrifiable materials, unites with them into a glass, which, although easily fusible, is not in the least corroded or acted on by any articles of food. It is not, therefore, from the apprehension of any injury to the health of those who use vessels of porcelain, that the use of lead in the glaze is objectionable, but because it is extremely liable to combine with and degrade the more delicate colours, especially those given by preparations of chrome and of gold. This is particularly the case in the more expensive and elaborate products which, on account of the multiplicity of their colours, require to be repeatedly heated, or *fired*. I trust, therefore, that the Society will consider the communication of a receipt for glazing, in which the abovementioned defects are avoided, as worthy of their favourable notice.

The principal ingredient of my glaze is felspar, of a somewhat compact texture, and a pale flesh red colour, which forms veins in a slaty rock adjoining to the town of Welsh Pool in Montgomeryshire. This material, being freed from all adhering pieces of slate and of quartz, is ground to a fine powder, and being thus prepared, I mix with 27 parts of felspar, 18 of borax, 4 of Lynn sand, 3 of nitre, 3 of soda, and 3 of Cornwall China clay. This mixture is to be melted to a frit, and is then to be ground to a fine powder, 3 parts of calcined borax being added previously to the grinding.

The specimens accompanying this letter, are,

1. The felspar in its rough state.
2. Do. ground to a fine powder.
3. Some of the glaze ready prepared for dipping.
4. Specimens of porcelain glazed.
5. Do. both glazed and afterwards painted, in order to show the solidity and brilliancy of the colours when used on this glaze.

I am, sir, &c.

A. Aikin, Esq., Sec. &c. JOHN ROSE.

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1820. The Isis gold medal of the Society was voted to Mr. Rose for this communication.

Some

Some of the specimens furnished by Mr. Rose were placed by the committee in the hands of Mr. Muss, and of other artists, in order to be submitted to experiment both with regard to the perfection of the glaze itself at high temperatures, and its reaction on the several colouring materials.

Mr. Muss's trial pieces were proved first in a common kiln, and afterwards were subjected to the action of a much higher degree of heat than it is possible that they can ever be exposed to in the fair course of enamelling. In this extreme heat the ground of the ware is not in the least degree softened or affected; the glaze remains firm and perfectly uniform without any specks or spits having been produced on its surface; the colours, even the pinks and chrome greens, come out remarkably well upon it. Mr. Rose's glaze not being so hard as that used by the French manufacturers, incorporates more completely with the colours, and renders them perfectly firm; whereas every artist knows that colours laid on French porcelain are extremely apt to chip off, crackle, and flake, if it is necessary to make them pass the fire a second time. On the whole, therefore, Mr. Muss considers the samples placed in his hands by the committee, as the best both in body and glaze that have ever come under his observation.

Similar reports of the excellency of the glaze, in the particulars above mentioned, were made by the other artists who had made trial of it.

XLVI. *Report of the Council of the Astronomical Society of London to the first Annual General Meeting Feb.* 9, 1821.

In making this first report of their proceedings, the Council cannot but congratulate the Members on the success which has attended the first attempt to establish, in this country, a Society for the promotion of so important a branch of science as Astronomy.

Notwithstanding the difficulties and delays usually attending the establishment of every institution, the efforts of the founders of this Society, have been crowned with an accession of strength far beyond their most sanguine expectations; and which seems likely still to increase. Meanwhile, adhering steadily to the principles laid down in the Address, circulated at its first institution, "of avoiding all interference with the objects and interests of other established scientific bodies," it cannot but be gratifying in the highest degree to have observed a reciprocity of feeling on this subject, recently expressed from the chair of the most eminent scientific institution which this or any other country can boast.

In the infancy of the Society much of the time of the Council has been necessarily employed in arranging the usual routine of its business: nevertheless, they venture to hope that, in those subjects which have come before them, they have laid the basis for promoting future improvements and discoveries; which, followed up with zeal and assiduity, must lead to the most beneficial results.

In the science of Astronomy, where the observations of two or three thousand years, and those made in various parts of the world, have not yet led to the rigorous determination of many of the elements of the science, it can scarcely be expected that the efforts of a few individuals, hitherto confined to their own country, can have effected much in the short space of a twelvemonth. But, since more can *now* be done in the compass of a few years than could be formerly effected in a century (owing in a great measure to the superior accuracy of modern instruments, but more especially to those invaluable principles of philosophy and that refined analysis first introduced by our illustrious countryman) they indulge the pleasing but not unreasonable hope that, by the active co-operation of a whole scientific body, and the zeal and emulation thereby excited among astronomers of every country, the science will advance constantly and more rapidly towards that state of ultimate perfection, which it is so eminently calculated to attain.

With a view to stimulate such pursuits, the Council have ordered a die to be formed, for the purpose of striking medals in bronze, silver and gold; to be bestowed, as an honorary distinction, on such persons as may, from time to time, distinguish themselves by any material discovery, or improvement in the science. And, in order to direct the attention of astronomers to those points which appear most worthy of encouragement, they will here state some of the principal subjects on which they have at present decided to bestow such rewards. In the first place, they propose to bestow the medal for the discovery of any new planet, satellite, or comet: or for the re-discovery of any old comet, or of any stars that have disappeared. Considering also the great importance (both in a nautical and in a geographical point of view) of having accurate observations of the eclipses of Jupiter's satellites and of occultations of stars by the moon, they think that the medal should be given for any considerable collection not only of original observations of this kind, but also of well authenticated recorded observations, reduced to the mean time of the meridian of some well known observatory. Observations likewise on the positions of the fixed stars, tending either to the enlargement and perfection of our present catalogues, or to the more accurate determination of the variable ones in size,

colour,

colour, or situation;—as well as observations on double stars, tending, in like manner, not only to the enlargement and perfection of the present catalogues, but also to the determination of their angular distance, and of their angle of position;—together with observations on nebulæ—appear proper subjects of such reward. To these may be added, observations on refraction, with a view to the more perfect theory of that phænomenon; particularly at low altitudes, where irregularities take place, when little or no variation has taken place in the barometer or thermometer:—observations on the tides, particularly in situations where the current is not influenced by any contiguous continent, as will be more fully alluded to in the sequel:—observations tending to determine the true figure of the sun, or of the earth:—and, in short, any observations which may be considered likely to advance and improve the science.

But, it is not to observations alone that the Council would wish to confine the bestowing of the Society's medal. The reduction of observations when made is another and oftentimes a more laborious task: and, without the latter, the former would be of little or no service to the astronomer. To this subject, therefore, the Council wish to invite the attention of the computer; as well as to the formation of more simple and easy tables, for the reduction of astronomical observations, than those at present in existence. The formation of new tables for the more recently discovered planets, as well as more accurate tables of the sun, moon and other planets, together with those of Jupiter's satellites, is a subject too important to need the recommendation of this Society. The comparison likewise of the places of any of these bodies, observed in the present century at any of the principal observatories, with their places deduced from the most approved tables, but more particularly those of the moon, is an object worthy of encouragement. In the latter case, however, it would be desirable that the numerical value of the arguments of the principal equations should be annexed to each comparison: and that, in all cases, the principles on which the deductions are made should be fully and clearly stated. But, independent of these subjects, there are many other useful tables tending to facilitate astronomical calculations, some of a permanent and others of a temporary or local nature, which would be a great assistance to the practical astronomer, and worthy the patronage of this Society. And, without particularizing such subjects, the Council wish it to be understood that these are amongst the objects which they are desirous to reward with the Society's medal.

The Council likewise wish to direct the attention of the diligent inquirer to the recorded observations of preceding astronomers, not only with a view to discover whether any observations are to

be there found of any of the more recently discovered planets or comets, but also with a view to the formation of a more complete catalogue of such stars as have, from time to time, disappeared from our sight. These subjects, together with accurate and descriptive accounts of the instruments used by eminent deceased observers, in order to estimate the reliance to be placed on their recorded observations, might fairly claim some mark of distinction.

With respect to instruments, the Council propose to bestow the medal for every improvement which may tend materially to advance the science. They would mention however, as a few amongst the desiderata, an instrument for determining the apparent magnitudes of the stars, or of ascertaining a correct scale whereby astronomers may be enabled to express themselves in one common language on this subject. Likewise a simple but effectual contrivance for enabling an observer to determine the right ascension and declination of small stars, without the necessity of illuminating the field of the telescope. And a method of applying the reflecting telescope to transit or circular instruments, in as convenient and useful a manner as the refracting telescope.

It would be impossible in a report of this kind to enumerate *all* the subjects which may be considered worthy of the Society's medal: neither can the Council establish any general scale for the precise distribution of the three kinds which it is intended to strike. Doubts may frequently arise on subjects of this nature: but, they trust they shall always act with liberality and impartiality, yet at the same time with a due regard to the dignity and character of the Society, and the nature of the trust reposed in them. It may indeed appear extraordinary that no mention should yet have been made of the great desiderata of astronomy,—those questions which have exercised the curiosity and employed the time and attention of astronomers ever since the science has assumed its present character—such as the parallax of the fixed stars, their proper motion, the motion or rest of our own system, and its connection with the rest of the universe. But these and many other points are too obviously suggested by their importance to need any particular notice or encouragement. The man, for whom discoveries of this class are reserved, soars far beyond any distinction which this Society can bestow: the applause of the human race attends his labours; and no additional stimulus can be offered to those by which he is impelled.

The subjects of physical astronomy are so various, and of so mixed and complicated a nature, that the Council defer to a future opportunity their observations on this head. In order, however, to show their disposition to encourage the consideration of such

such subjects (unhappily too much neglected among the geometers of this country) they recommend the proposal of the Society's gold medal and twenty guineas for the solution of the following prize question:

> "For the best paper on the theory of the motions and perturbations of the satellites of Saturn. The investigation to be so conducted as to take expressly into consideration the influence of the rings, and the figure of the planet as modified by the attraction of the rings, on the motions of the satellites: to furnish formulæ, adapted to the determination of the elements of their orbits, and the constant coefficients of their periodical, and secular equations, from observation: likewise to point out the observations best adapted to lead to a knowledge of such determination. The papers to be sent to the Society on or before the 1st day of February 1823."

And, in order to conceal the name of any unsuccessful competitor, the Council propose that each memoir should bear a motto; and that a sealed paper, bearing the same motto, contain the name of the author. In such case, the name of the successful candidate only will be divulged, and the sealed papers of the rest will be destroyed, unopened, in the presence of the Council. The successful paper must be left with the Society, to be published as they may direct; and the rest will be returned on proper application of the authors.

The pecuniary resources of the Society, although of course not large, are sufficient to answer every expense which may be incurred by the adoption of the plans recommended by the Council. The number of resident members is 82; out of which, 12 have compounded for their annual payments; and there are 37 non-resident members: so that the total assets of the Society arising from this source are 907*l.* 4*s.* together with a present annual income of about 140*l.* subject to a further increase from the acquisition of new members. The report of the auditors will state the sums actually received, and the sums paid by the Society, together with the balance remaining. It may be proper however here to mention that the whole of the compositions are, by a resolution of the Council, to be invested from time to time in the Navy 5 per cents. in the joint names of the trustees; and that the payments of the non-resident members are at present invested in East India Bonds with a view to a similar permanent investment: it being conceived that these two sources of income should be kept distinct from the annual payments.

The expenses of the Society, up to the present time, have been very trifling, principally owing to the very liberal spirit exhibited by the Geological Society, in granting the use of their commodious apartments for the meetings of the Council and the Members

bers of this Society, in the very infancy of its existence: and for which, this Society is bound to retain a due and grateful remembrance.

At the close of the last session, the Council received a communication from Captain Basil Hall, expressing his readiness to attend to any instructions on subjects wherein he might be of service to the science of Astronomy, in his intended voyage to the South Seas. They availed themselves of the offer of this intelligent and enterprizing officer, and requested his attention to the following principal points.

To observe, as frequently as possible, the conjunctions of the moon and planets with the fixed stars; measuring with a micrometer their differences of right ascension and declination, or taking the measure of their distance in a straight line: the time and place of observation being correctly noted.

To look out for occultations of the fixed stars by the moon; and particularly for those which may be presumed to be of short duration, with a view to the illustration of the theory of Cagnoli respecting this mode of determining the figure of the earth. And it was remarked to him that, as the moon was now, and would be for some few years, in such a position with respect to her nodes as to pass over the *Pleiades* every lunation, it would be particularly desirable to look out for the occultations of those stars.

To make frequent sweeps of the heavens, with a telescope having a large field of view and small magnifying power, for the purpose of discovering any comets; and to note the progress and circumstances of the same; making sketches of their appearance. The Council at that time were not in possession of the calculated place of the comet which is expected to return in 1822. But, having since received an ephemeris of its apparent positions, computed by M. Encke, they will endeavour to forward it to Captain Hall, and request him to look out for the same: a circumstance the more to be desired, since it is expected to assume a different appearance in the southern hemisphere to that which it will present in Europe.

It appeared unnecessary to remind Captain Hall of the several eclipses of the sun and moon, together with the eclipses of Jupiter's satellites, the transit of Mercury over the sun's disc on November 4, 1822, and the several phænomena noted in the various ephemerides; and which would of course be the object of his attention without any particular suggestions from this Society. But the Council took the liberty of directing his attention to certain points, should he be favourably situated for observing any of the solar eclipses, or the transit of Mercury.

They also requested him to make observations on the position of Mars, at the time of his opposition in February 1822, with respect

respect to the three following fixed stars, which are situated near the path of his orbit, and whose mean places for the day of opposition are here stated: viz.

Star.	Æ. in time.	N. Dec.
	h ′ ″	° ′ ″
42 Leonis	10 . 12 . 15,9	15 . 52 . 11,9
446 Mayer	19 . 17,6	15 . 1,0
i 46 Leonis	22 . 41,6	2 . 53,3

When Mars approaches either of these stars, the differences in right ascension and declination, between the planet and the star, must be taken as accurately as possible for several successive days, with a micrometer; or their distances measured, in a straight line: the time and place being correctly noted down. Such observations, compared with corresponding ones made in England, will serve to determine the parallax of Mars.

With the same view it was proposed to Captain Hall, to make observations on Venus, at the time of her inferior conjunction in March 1822, by comparing her with α Ceti, on the parallel of which she will be a day or two before and after the conjunction: a simple and easy method being at the same time suggested, whereby Venus might be readily found in the day time, notwithstanding her proximity to the sun.

The attention of Captain Hall was also directed to the subject of refraction, in order to determine whether the quantity of refraction varies (*cæteris paribus*) in different parts of the globe; or whether any new light can be thrown on this uncertain phænomenon, in the various places he might visit.

Upon most of these subjects, it must be evident to the members of the Society, that there is a necessity of having corresponding and simultaneous observations in this country: otherwise the labours of Captain Hall (should he be favourably situated for observation) will be in a great measure lost to the public. Those members, therefore, who possess the requisite instruments, are called upon to co-operate on these points, and to register their observations, in order that they may be compared at a future opportunity. Without this assistance the efforts of the Council will have been exerted in vain, and the time of an active observer employed to little or no advantage. And here the Council cannot avoid suggesting, to those astronomers who possess the requisite instruments, the propriety of observing and recording the position (in right ascension and *declination*) of those stars which are situated near to, and on the same parallel with, any of the planets near the time of their opposition; since such observations would

would serve as a mode of comparison for those observers who, with less powerful instruments, might be more favourably situated for making observations at this important point of the orbit of the planets.

The Council further represented to Captain Hall that it was scarcely to be expected that a traveller, who is frequently changing his situation, can make may *fundamental* observations in astronomy; such as may serve as a basis for future researches. But, that he might do much in *comparative* astronomy; by taking those elements as correct which have been determined by observations made in fixed observatories, and comparing the objects of research therewith: some examples of which have been already alluded to; and others were suggested for his consideration.

But there was still another, and a very important point, to which the attention of Captain Hall was requested (and the same cannot be too strongly pressed on any future voyager, or settler in distant climates, favourably situated for such inquiries): namely, to make regular observations on the tides, in favourable situations for determining their theory. It is well known that the tides, adjoining large continents and their contiguous islands, are so affected with the various sources of error arising from the situation of the harbour and the nature of the bottom of the ocean for a considerable distance around it, that not only a very long series of observations is required to destroy or compensate those errors, and allow the true coefficients of the formulæ, for determining their value, to appear with any tolerable exactness; but also the coefficients themselves, so determined, are essentially affected by such local peculiarities; and consequently incapable of affording any thing beyond relative results. In order to obtain results unaffected with these inappreciable causes of error, the places of observation should, if possible, be chosen on small islands shooting up abruptly from an unfathomable depth in the midst of a wide ocean, extending 30 or 40 degrees, at least, in all directions; or, at all events, a very great distance from any large continent. The islands in the Pacific and South Atlantic oceans, which are bedded on coral banks or the effect of volcanic eruptions, are precisely of this nature. If we may trust the accounts of voyagers, many of these are mere vertical shafts, or insulated columns, shooting at once from the very bottom of the ocean, without shoals, or any gradual declivity. Round these, the tides must rise and fall with perfect uniformity: and it is exceedingly probable that, in these cases, a much shorter series of observations would be requisite for framing accurate results: and that even those of a single month, in moderately calm weather, might have considerable value in the present improved state of the theory. The situation of the Gallapagos islands, on which Cap-

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tain Hall will probably spend some time (it being one of the stations at which he proposes to swing the invariable pendulum), possesses peculiarities which entitle it to notice, although it does not satisfy all the conditions. It is immediately under the equator: and should he be there about the time of the equinox, the very vertex of the aqueous spheroid, which will then pass over the spot, may be made the subject of his observations. These islands likewise present another remarkable peculiarity of situation. For, according to the results obtained from a theorem given by M. Biot, they stand within a very few degrees of the point where the magnetic intersects the terrestrial equator. It is therefore desirable that observations should be made with a view to ascertain the accuracy of this conclusion. It may also be remarked, that it is near this spot that the magnetic equator is supposed to deviate into the serpentine form, as mentioned by the same eminent writer.

The formation of an Astronomical Library being one of the objects of the Society, the attention of the Council has been directed thereto. At present, however, they have not thought it advisable to expend any money on this department. Nevertheless they are happy in announcing that the foundation has been laid, by the donation of many valuable works on the science by several members of the Society.

It having been represented to the Council, that the observations made at the observatory of the East India Company at Madras were preserved in the library of the East India Company, application was made for them to the Court of Directors. With a truly laudable zeal for the cause of science, the Court immediately assented to this request; and ordered not only that the present observations for a series of years (commencing with the year 1793, and continued with some slight interruptions to the present time) hould be deposited with this Society, but that the future observations should be forwarded in like manner. Those observations, accompanied with many other valuable papers on astronomical subjects from the same quarter, are now in the library of the Society.

Since the last general meeting of the Society the Council have considered the expediency of recommending a few alterations in the Regulations. And they have unanimously agreed to propose: First, that no person, who has filled the office of President for two successive years, should be again eligible to the same situation, until the expiration of one year from the termination of his office; similar to the present regulation respecting the Vice-presidents: Secondly, that the six members of council alluded to in Section 2, Regulation 2, should be extended to eight, and consequently that four should be re-eligible in every ensuing year, instead

instead of three only: Thirdly, that any person who may be desirous of reading his own paper to the Society, may be at liberty so to do: Lastly, that the mode of calling special general meetings should be so far altered, as to direct that the subject to be discussed at such special meetings shall be previously laid before the Council; and, if the decision of the Council be not satisfactory to the members proposing the subject, to make it imperative on the Council to call a special general meeting within a given period. Distinct resolutions, on each of these points, will be submitted for your consideration *.

During the last year the Council have considered the propriety of appointing Committees for various purposes: amongst others, one for determining on a set of questions to be proposed to persons possessing astronomical instruments, in order to ascertain the merits of the same; and another to determine on the expediency of procuring tables of the apparent places of the 46 Greenwich stars for every day in the year. But, at present, nothing decisive has been effected on these points.

One of the objects of this Society being an examination of the heavens in minute detail, the Council have likewise frequently discussed this subject, but without being able to agree on a plan, proper to be recommended for the adoption of the members. They consider it, however, a subject of so much importance, that they will early resume it: for, until every remarkable star in the heavens is recorded, and its place assigned in the catalogue, it is vain to pretend to an accurate knowledge of the true system of the universe.

It is gratifying to observe that the advantages, likely to accrue to science from the establishment of a Society like this, appear to be duly appreciated by the continental astronomers. From many of them (whose names, among the most illustrious in modern science, now stand on our list as associates, or are suspended in our meeting room as candidates for admission) letters have been received by the foreign secretary, expressive not merely of the most unqualified approbation of our objects, but of a desire to co-operate actively in their accomplishment: and which has been evinced in more than one instance by the communication of interesting notices, and astronomical works.

On the whole, the Council cannot view this new impulse which appears to have been given to astronomy in all parts of the world, without anticipating the most beneficial results to the science. The establishment of several new observatories on the continent of Europe (one of them above the 60th degree of north latitude) under the direction of men eminent in science, and vieing with

* These resolutions were carried unanimously; and the Regulations have been altered accordingly in the new edition just published.

each

each other in the most honourable branch of emulation—the rising efforts of our countrymen in the East Indies—the zeal of our brethren on the American continent—the foundation of a public observatory at Cambridge and another at the Cape of Good Hope (both so honourable to our own country)—must ensure the good wishes of every friend to science, and excite the admiration of every reflecting mind.

The following is a List of the Papers which have been read at the Meetings of the Society, in the preceding year.

March 10, 1820. On the doubly refracting property of rock crystal, considered as a principle of micrometrical measurements when applied to a telescope. By the Rev. Dr. Pearson.

April 14. On the construction and use of a new micrometrical eye piece of a telescope. By the same.

May 12. A letter from G. Peacock, Esq. to C. Babbage, Esq. respecting the intended astronomical observatory at Cambridge.

On double stars: with a Catalogue of the same. By J. South, Esq.

June 9. On a method of fixing a transit instrument exactly in the meridian: with Tables applicable to the same. By F. Baily, Esq.

Nov. 10. Notice respecting the occultation of the Pleiades, in the ensuing years 1821, 1822, 1823, and 1824.

Universal tables for the reduction of the fixed stars. By S. Groombridge, Esq.

Translation of a printed notice respecting a new meridian circle lately erected at the Observatory at Gottingen; transmitted by M. Gauss.

Dec. 8. An Ephemeris of Vesta. By S. Groombridge, Esq.

On the late solar eclipse, September 7, 1820. By F. Baily, Esq.

Jan. 12, 1821. A short account of the repeating circle, and of the Altitude and Azimuth instrument: describing their different constructions, the means of performing their principal adjustments, and how to make observations with them. By E. Troughton, Esq.

XLVII. *A Me-*

XLVII. *A Memoir on some new Modifications of Galvanic Apparatus, with Observations in Support of his Theory of Galvanism.* *By* R. Hare, *M.D. Professor of Chemistry in the University of Pennsylvania**.

I had observed that the ignition produced by one or two galvanic pairs attained its highest intensity, almost as soon as they were covered by the acid used to excite them, and ceased soon afterwards; although the action of the acid should have increased during the interim. I had also remarked, in using an apparatus of three hundred pairs of small plates, that a platina wire of Number 16, placed in the circuit, was fused in consequence of a construction which enabled me to plunge them all nearly at the same time. It was therefore conceived, that the maximum of effect in Voltaic apparatus of extensive series had never been attained. The plates are generally arranged in distinct troughs rarely containing more than twenty pairs. Those of the great apparatus of the Royal Institution, employed by Sir H. Davy, had only ten pairs in each. There were one hundred such to be successively placed in the acid, and the whole connected ere the poles could act. Consequently the effect which arises immediately after immersion, would be lost in the troughs first arranged, before it could be produced in the last; and no effort appears to have been made to take advantage of this transient accumulation of power, either in using that magnificent combination, or in any other of which I have read. In order to observe the consequence of simultaneous immersion with a series sufficiently numerous to test the correctness of my expectations, a galvanic apparatus of eighty concentric coils of copper and zinc, was so suspended by a beam and levers, as that they might be made to descend into, or rise out of the acid in an instant. The zinc sheets were about nine inches by six, the copper fourteen by six; more of this metal being necessary, as in every coil it was made to commence within the zinc, and completely to surround it without. The sheets were coiled so as not to leave between them an interstice wider than a quarter of an inch. Each coil is in diameter about two inches and a half, so that all may descend freely into eighty glass jars two inches and three quarters diameter inside, and eight inches high, duly stationed to receive them.

My apparatus being thus arranged, two small lead pipes were severally soldered to each pole, and a piece of charcoal about a quarter of an inch thick, and an inch and a half long, tapering a little at each extremity, had these severally inserted into the

* Communicated by the Author.

hollow

hollow ends of the pipes. The jars being furnished with diluted acid and the coils suddenly lowered into them, no vestige of the charcoal could be seen. It was ignited so intensely, that those portions of the pipes by which it had been embraced were destroyed. In order to avoid a useless and tiresome repetition, I will here state that the coils were only kept in the acid while the action at the poles was at a maximum in the experiment just mentioned, and in others which I am about to describe, unless where the decomposition produced by water is spoken of, or the sensation excited in the hands. I designate the apparatus with which I performed them, as the galvanic deflagrator, on account of its superior power, in proportion to its size, in causing deflagration; and as, in the form last adopted, it differs from the voltaic pile in the omission of one of the elements heretofore deemed necessary to its construction.

Desirous of seeing the effect of the simultaneous immersion of my series upon water, the pipes soldered to the poles were introduced into a vessel containing that fluid. No extraordinary effect was perceived, until they were very near, when a vivid flash was observed, and happening to touch almost at the same time, they were found fused and incorporated at the place of contact. I next soldered to each pipe a brass cylinder of about five-tenths of an inch bore. These cylinders were made to receive the tapering extremities of a piece of charcoal about two inches long so as to complete the circuit. The submersion of the coils caused the most vivid ignition in the coal. It was instantaneously and entirely on fire. A piece of platina of about a quarter of an inch diameter in connexion with one pole, was instantly fused at the end on being brought in contact with some mercury communicating with the other. When two cylinders of charcoal having hemispherical terminations were fitted into the brass cylinders and brought nearly into contact, a most vivid ignition took place, and continued after they were removed about a half or three quarters of an inch apart, the interval rivalling the sun in brilliancy. The igneous fluid appeared to proceed from the positive side. The charcoal in the cylinder soldered to the latter would be intensely ignited throughout when the piece connected with the negative pole was ignited more towards the extremity approaching the positive. The most intense action seems to arise from placing a platina wire of about the eighth of an inch diameter, in connexion with the positive pole, and bringing it in contact with, and afterwards removing it a small distance apart from a piece of charcoal (fresh from the fire) affixed to the other pole.

As points are pre-eminently capable of carrying off (without being injured) a current of the electrical fluid, and very ill qualified to conduct caloric; while, by facilitating radiation, charcoal favours

vours the separation of caloric from the electricity which does not radiate; this result seems consistent with my hypothesis, that the fluid as extricated by Volta's pile is a compound of caloric and electricity*; but not with the other hypothesis, which supposes it to be electricity alone. The finest needle is competent to discharge the product of the most powerful machine without detriment, if received gradually as generated by them. Platina points, as small as those which were melted like wax in my experiments, are used as tips to lightning rods without injury; unless in sudden discharges produced under peculiar circumstances †.

The following experiment I conceive to be very unfavourable to the idea that galvanic ignition arises from a current of electricity.

A cylinder of lead of about a quarter of an inch diameter, and about two inches long, was reduced to the thickness of a common brass pin for about three quarters of an inch. When one end was connected with one pole of the apparatus, the other remained suspended by this filament; yet it was instantaneously fused by contact with the other pole. As all the calorific fluid which acted upon the suspended knob, must have passed through the filament by which it hung, the fusion could not have resulted from a pure electrical current, which would have dispersed the filament ere a mass fifty times larger had been perceptibly affected. According to my theory, caloric is not separated from the electricity until

* According to the theory here alluded to, the galvanic fluid owes its properties to caloric and electricity; the former predominating in proportion to the size of the pairs, the latter in proportion to the number, being in both cases excited by a powerful acid. Hence in batteries which combine both qualifications sufficiently, as in all those intervening between Children's large pairs of two feet eight inches by six feet, and the 2000 four-inch pairs of the Royal Institution, the phænomena indicate the presence of both fluids. In De Luc's column, where the size of the pairs is insignificant, and the energy of interposed agents feeble, we see electricity evolved without any appreciable quantity of caloric. In the calorimotor where we have size only, the number being the lowest possible, we are scarcely able to detect the presence of electricity.

When the fluid contains enough electricity to give a projectile power adequate to pass through a small space in the air, or through charcoal, which impedes or arrests the caloric, and favours its propensity to radiate, this principle heat is evolved. This accounts for the evolution of intense heat under those circumstances which rarifies the air, so that the length of the jet from one pole to the other may be extended after its commencement. Hence the portions of the circuit nearest to the intervening charcoal, or heated space, are alone injured; and even non-conducting bodies, as quartz, introduced into it are fused, and hence a very large wire may be melted by the fluid, received through a small wire imperceptibly affected.

See Silliman's Journal, No. 6, Vol. 1. Thomson's Annals, Sept. 1810. Tilloch's Philosophical Magazine, October 1819.

† See Adams's Electricity, On points.

circum-

circumstances very much favour a disunion, as on the passage of the compound fluid through charcoal, the air, or a vacuum. In operating with the deflagrator, I have found a brass knob of about five tenths of an inch in diameter, to burn on the superficies only; where alone, according to my view, caloric is separated so as to act on the mass. Having, as mentioned in the memoir on my theory of galvanism, found that four galvanic surfaces acted well in one recipient, I was tempted by means of the eighty coils to extend that construction. It occurred to me that attempts of this kind had failed from using only one copper for each zinc plate. The zinc had always been permitted to react towards the negative as well as the positive pole. My coils being surrounded by copper, it seemed probable, that, if electro-caloric were, as I had suggested, carried forward by circulation arising from galvanic polarity, this might act within the interior of the coils, yet not be exerted between one coil and another.

I had accordingly a trough constructed with a partition along the middle, so as to receive forty coils on one side, and a like number on the other. This apparatus when in operation excited a sensation scarcely tolerable in the backs of the hands. Interposed charcoal was not ignited as easily as before; but a most intense ignition took place on bringing a metallic point connected with one pole of the series, into contact with a piece of charcoal fastened to the other. It did not take place, however, so speedily as when glasses were used; but soon after the ignition was effected it became even more powerful than before. A cylinder of platina nearly a quarter of an inch in diameter, tapering a little at the end, was fused, and burned so as to sparkle to a considerable distance around, and fall in drops. A ball of brass of about half an inch diameter was seen to burn on its surface with a green flame. Tin foil, or tinsel rolled up into large coils of about three quarters of an inch thick, were rapidly destroyed, as was a wire of platina of No. 16. Platina wires in connexion with the poles were brought into contact with sulphuric acid; there was an appearance of lively ignition, but strongest on the positive side. Excepting in its power of permeating charcoal, the galvanic fluid seemed to be extricated with as much force as when each coil was in a distinct glass. Apprehending that the partition in the trough did not sufficiently insulate the poles from each other, as they were but a few inches apart, moisture or moistened wood intervening, I had two troughs made, each to hold forty pairs, and took care that there should be a dry space about four inches broad between them. They were first filled with pure river water, there being no saline nor acid matter to influence the plates, unless the very minute quantity which might have remained on them from former immersions. Yet the sensation produced

duced by them, on the backs of my hands, was painful; and a lively scintillation took place when the poles were approximated. Dutch gold leaf was not sensibly burned, though water was found decomposable by wires properly affixed. No effect was produced on potash, the heat being inadequate to fuse it.

A mixture of nitre and sulphuric acid was next added to the water in the troughs, afterwards charcoal from the fire was vividly ignited; and when attached to the positive pole a steel wire was interposed between it and the other pole, the most vivid ignition which I ever saw was induced. I should deem it inprudent to repeat the experiment without glasses, as my eyes, though unusually strong, were affected for forty-eight hours afterwards. If the intensity of the light did not produce an optical deception, by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion.—That charcoal should be thus softened, without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise, the air could have no access, first, he causeof the excessive rarefaction, and in the next place, as I suspect, on account of the volatilization of the carbon forming about it a circumambient atmosphere. This last-mentioned impression arose from observing, that when the experiment was performed *in vacuo*, there was a lively scintillation, as if the carbon in an aëriform state acted as a supporter of combustion on the metal.

A wire of platina (No. 16) was fused into a globule on being connected with the positive pole, and brought into contact with a piece of pure hydrate of potash, situated on a silver tray in connexion with the other pole. The potash became red hot, and was deflagrated rapidly with a flame having the rosy hue of potassuretted hydrogen.

The great apparatus of the Royal Institution, *in projectile power* was from six to eight times more potent than mine. It produced a discharge between charcoal points when removed about four inches apart, whereas mine will not produce a jet at more than three fourths of an inch. But that series contained 2000 pairs, mine is only about a twenty-fifth part as large.

A steel wire of about one tenth of an inch in diameter, affixed to the negative pole, was passed up through the axis of an open necked inverted bell glass filled with water. A platina wire, No. 16, attached to a positive pole, being passed down to the steel wire, both were fused together, and, cooling, could not be separated by manual force.—Immediately after this incorporation of their extremities, the platina wire became incandescent for a space of some inches above the surface of the water.

A piece

A piece of silvered paper about two inches square was folded up, the metallic surface outward, and fastened into vices affixed to the poles. Into each vice a wire was screwed at the same time. The fluid generated by the apparatus was not perceptibly conveyed by the silvered paper, as it did not prevent the wires severally attached to the poles from decomposing water or producing ignition by contact.

In my memoir on my theory of galvanism I suggested, that the decomposition of water, which Wollaston effected by mechanical electricity, might not be the effect of divellent attraction like those excited by the poles of a voltaic pile, but of a mechanical concussion, as when wires are dispersed by the discharge of an electrical battery. In support of that opinion I will now observe, that he could not prevent hydrogen and oxygen from being extricated at each wire, instead of hydrogen being given off only at one, and oxygen at the other, as is invariably the case when the voltaic pile is employed. That learned and ingenious philosopher, in concluding his account of this celebrated experiment, says, "but in fact the resemblance is not complete, for in every way in which I have tried it, I observed each wire gave out both oxygen and hydrogen gas, instead of their being formed separately as by the electric pile."

Is it not reasonable to suppose that an electrical shock may dissipate any body into its elementary atoms, whether simple or compound, so that no two particles would be left together which can be separated by physical means?

Looking over Singer's Electricity, a recent and most able modern publication, I find that in the explosion of brass wire by an electrical battery, the copper and zinc actually separated. He says, page 186, "Brass wire is sometimes *decomposed* by the charge; the copper and zinc of which it is formed being separated from each other, and appearing in their distinct metallic colours." In the next page of the same work, I find that the oxides of mercury and tin are reduced by electrical discharges. "Introduce," says the author, "some oxide of tin into a glass tube, so that when the tube is laid horizontal, the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lie between them. Pass several strong charges in succession through the tube, replacing the oxide in its situation, should it be *dispersed*. If the charges are sufficiently powerful, a part of the tube will soon be stained with metallic tin which has been revived by the action of transmitted electricity." It cannot be alleged that in such decompositions the divellent polar attractions are exercised like those which characterize the action of wire

proceeding

proceeding from the poles of a voltaic apparatus. The particles were dispersed from, instead of being attracted to the wires, by which the influence was conveyed among them. This beng undeniable, it can hardly be advanced that we are to have one mode of explaining the separation of the elements of brass by an electrical discharge, another of explaining the separation of the elements of water by the same agent; one *rationale* when oxygen is liberated from tin, and another when liberated by like means from hydrogen. In the experiment in which copper was precipitated by the same philosopher at the negative pole, we are not informed whether the oxygen and acid in union with it were attracted to the other; and the changes produced in litmus are mentioned not as simultaneous, but successive. The violet and red rays of the spectrum have an opposite chemical influence in some degree like that of voltaic poles, but this has not led to the conclusion that the cause of galvanism and light is the same. Besides admitting that the feeble results obtained by Wollaston and Van Marum with electrical machines, are perfectly analogous to those obtained by the galvanic pile, ere it can become an objection to my hypothesis, it ought first to be shown that the union between caloric and electricity, which I suppose productive of galvanic phænomena, cannot be produced by that very process. If they combine to form the galvanic fluid when extricated by ordinary galvanic action, they must have an affinity for each other. As I have suggested in my memoir, when electricity enters the pores of a metal, it may unite with its caloric. In Wollaston's experiments, being constrained to enter the metal, it may combine with enough of its caloric to produce, when emitted, results slightly approaching to those of a fluid in which caloric exists in greater proportion.

But once more I demand, Why, if mechanical electricity be too intense to produce galvanic phænomena, should it be rendered more capable of producing them by being still more concentrated?

If the one be generated more copiously, the other more intensely, the first will move in a large stream slowly, the last in a small stream rapidly. Yet by narrowing the channel of the latter, Wollaston is supposed to render it more like the former, that is, produces a resemblance by increasing the supposed source of dissimilarity.

It has been imagined that the beneficial effect of his contrivance arises from the production of a continued stream, instead of a succession of sparks; but if a continued stream were the only desideratum, a point placed near the conductor of a powerful machine would afford this requisite, as the whole product may in such cases be conveyed by a sewing needle in a stream perfectly continuous.

continuous. As yet no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the processes of Van Marum and Wollaston, to inclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity is accounted for, according to my hypothesis, by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluid from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attractions which it may exercise, operates almost without restraint. This is fully exemplified in my galvanic deflagrator, where eighty pairs are suspended in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation* when excited only by fresh river water. I have already observed, that the reason why galvanic apparatus composed of pairs consisting each of one copper and one zinc plate, have not acted well without insulation †, was because electro-caloric could retrocede in the negative, as well as advance in the positive direction. I will now add, that independently of the greater effect produced by the simultaneous immersion of my eighty coils, their power is improved by the proximity of the surfaces, which are only about 3-16ths of an inch asunder; so that the circulation may go on more rapidly.

Pursuant to the doctrine, which supposes the same quantity of electricity, varying in intensity in the ratio of the number of pairs to the quantity of surface, to be the sole agent in galvanic ignition, the electrical fluid as evolved by Sir H. Davy's great pile must have been nearly two thousand times more intense than as evolved by a single pair, yet it gave sparks at no greater distance than the thirtieth or fortieth of an inch. The intensity of the fluid must be at least as much greater in one instance, than in another, as the sparks produced by it are longer. A fine electrical plate machine of thirty-two inches diameter, will give sparks at ten inches. Of course the intensity of the fluid which it emits, must

* I do not say shock, as it is more like the permanent impression of a pointed wire, and when acid is used a hot one.

† That is, with the same mass of conducting fluid, in contact with all the surfaces, instead of being divided into different portions, each restricted in its action to one copper and one zinc plate.

be

be three hundred times greater than that emitted by two thousand pairs. The intensity produced by a single pair, must be two thousand times less than that produced by the great pile, and of course six hundred thousand times less than that produced by a good electrical plate of thirty-two inches. Yet a single pair of about a square foot in area, will certainly deflagrate more wire than a like extent of coated surface charged by such a plate. According to Singer, it requires about one hundred and sixty square inches of coated glass, to destroy watch pendulum wire; a larger wire may be burned off by a galvanic battery of a foot square. But agreeably to the hypothesis in dispute, it compensates by quantity, for the want of intensity. Hence the quantity of fluid in the pair is six hundred thousand times greater, while its intensity is six hundred thousand times less; and *vice versâ* of the coated surface. Is not this absurd? What does intensity mean as applied to a fluid? Is it not expressed by the ratio of quantity, to space? If there be twice as much electricity within one cubic inch, as within another, is there not twice the intensity? But the one acts suddenly, it may be said; the other slowly. But whence this difference? They may both have exactly the same surface to exist in. The same zinc and copper plates may be used for coatings first, and a galvanic pair afterwards. Let it be said, as it may in truth, that the charge is, in the one case, attached to the glass superficies, in the other exists in the pores of the metal. But why does it avoid these pores in one case and reside in them in the other? What else resides in the pores of the metal which may be forced out by percussion? Is it not caloric? Possibly, unless under constraint, or circumstances favourable to a union between this principle and electricity, the latter cannot enter the metallic pores, beyond a certain degree of saturation; and hence an electrical charge does not reside in the metallic coatings of a Leyden phial, though it fuses the wire which forms a circuit between them.

It is admitted that the action of the galvanic fluid, is upon or between atoms; while mechanical electricity when uncoerced, acts only upon masses. This difference has not been explained unless by my hypothesis, in which caloric, of which the influence is only exerted between atoms, is supposed to be a principal agent in galvanism. Nor has any other reason been given that water, which dissipates pure electricity, should cause the galvanic fluid to accumulate. From the prodigious effect which moist air, or a moist surface, has in paralysing the most efficient machines, I am led to suppose, that the conducting power of moisture so situated, is greater than that of water under its surface. The power of this fluid to conduct mechanical electricity, is unfairly contrasted with that of a metal, when the former is inclosed in a glass tube, the latter bare.

Ac-

According to Singer, the electrical accumulation is as great when water is used, as when more powerful menstrua are employed; but the power of ignition is wanting, until these are resorted to. De Luc showed, by his ingenious dissections of the pile that electricity might be produced *without*, or *with*, chemical power. The *rationale* of these differences never has been given, unless by my theory, which supposes caloric to be present in the one case, but not in the other. The electric column was the fruit of De Luc's sagacious inquiries, and afforded a beautiful and incontrovertible support to the objections he made to the idea, that the galvanic fluid is pure electricity, when extricated by the voltaic pile in its usual form. It showed that a pile really producing pure electricity, is devoid of the chemical power of galvanism.

We are informed by Sir H. Davy, that when charcoal points, in connexion with the poles of the magnificent apparatus with which he operated, were first brought nearly into contact, and then withdrawn four inches apart, there was a heated arch formed between them, in which such non-conducting substances as quartz were fused. I believe it impossible to fuse electrics by mechanical electricity. If opposing its passage they may be broken, and if conductors near them be ignited, they may be acted on by those ignited conductors as if otherwise heated; but I will venture to predict, that the slightest glass fibre will not enter into fusion, by being placed in a current from the largest machine or electrical battery.

I am induced to believe, that we must consider light, as well as heat, an ingredient in the galvanic fluid; and think it possible, that, being necessary to vitality in animals, as well as vegetables, the electric fluid may be the vehicle of its distribution.

I will take this opportunity of stating, that the heat evolved by one galvanic pair has been found by the experiments which I instituted, to increase in quantity, but to diminish in intensity, as the size of the surfaces may be enlarged. A pair containing about fifty square feet of each metal, will not fuse platina, nor deflagrate iron, however small may be the wire employed; for the heat produced in metallic wires is not improved by a reduction in their size beyond a certain point. Yet the metals above mentioned are easily fused or deflagrated by smaller pairs, which would have no perceptible influence on masses that might be sensibly ignited by larger pairs.—These characteristics were fully demonstrated, not only by my own apparatus, but by those constructed by Messrs. Wetherill and Peale, and which were larger, but less capable of exciting intense ignition. Mr. Peale's apparatus contained nearly seventy square feet, Mr. Wetherill's nearly

nearly one hundred, in the form of concentric coils, yet neither could produce a heat above redness on the smallest wires. At my suggestion, Mr. Peale separated the two surfaces in his coils into four alternating, constituting two galvanic pairs in one recipient. Iron wire was then easily burned and platina fused by it. These facts, together with the incapacity of the calorific fluid extricated by the calorimotor to permeate charcoal, next to metals the best electrical conductor, must sanction the position I assigned to it as being in the opposite extreme from the columns of De Luc and Zamboni. For as, in these, the phænomena are such as are characteristic of pure electricity, so in one very large galvanic pair, they almost exclusively demonstrate the agency of pure caloric.

XLVIII. *Description of the Marine Thermometer Case invented by Mr.* Robert Jamieson, *of Glasgow**.

Glasgow, March 10, 1820.

Sir,—I beg leave, through you, to present for the inspection of the Society, a Marine Thermometer Case which I have constructed for Captain Livingston. The directions given to me by that gentleman, were, to make a case capable of preserving a thermometer from being broken when lowered into the sea, and drawn up again from the side of a ship; also, that it should admit water at any given depth, and retain it during the drawing up of the instrument, so as to enable a thermometer of the usual construction to indicate the temperature at different depths, undisturbed by the greater or inferior heat of the surface water.

The accompanying statement, drawn up by Captain Livingston and confirmed by his own experience, of the utility of thermometrical observations to the navigator, renders wholly unnecessary any remarks of mine on the same subject. I shall therefore confine myself to the mechanical construction of the instrument as exhibited in the drawing and model herewith transmitted. In order to answer its purpose, it was necessary to combine in the construction, strength, simplicity, and portability; but although these objects have been attained so as to produce a practically advantageous result, I am far from claiming for it absolute perfection. Longer experience and greater skill will probably be able to improve upon the present first attempt; and it is with this view, and also in order to call the public attention to a subject which promises to be highly beneficial to this maritime nation,

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1820. The large Silver Medal of the Society was voted to Mr. Jamieson for this communication.

tion, that I have taken the liberty of offering the instrument to the notice and encouragement of your patriotic Society.

I am, Sir, &c. &c. &c.

ROBERT JAMIESON.

Reference to the Engraving of Mr. JAMIESON'S *Marine Thermometer Case,* Plate II.

Fig. 5 is an exterior view of the instrument, and fig. 3 is a section of the same, with the inclosed thermometer.

The general form of the case is that of a cylinder or tube of copper, one-eighth of an inch in thickness, and seventeen inches in length; each end is open, and is bevelled off at the mouth for the more free admission of water.

v is the handle for the purpose of fixing it to the end of the cord. This handle is moveable on two pivots, by means of which it can readily be thrown on one side when it is necessary to take out or put in the thermometer.

At $2\frac{1}{8}$ inches from the top, is the circular joint *o o*, by means of which the lid forms a water-tight joint with the body of the case; the lid is also furnished with a hinge, on which it may be thrown back.

The valves are placed at each extremity of the tube, the upper one opening outwards, and the lower one inwards: they are composed of the following parts:

p q, a short cylinder or box, bevelled at each extremity.

s s, a bridge or cross bar, bulging at the middle, and there perforated, for the purpose of receiving an upright pin, to the upper end of which is fixed the circular plate of the valve, and to the lower end a screwed stud *t*, the round head of which prevents the pin of the valve from being drawn, by the pressure of the water, through the perforation in the cross bar.

r r, a circular projection, on which the bottom of the thermometer rests.

From this description, it is obvious that when the case containing the thermometer, and having its lid carefully closed, is dropped into the sea with a line attached to the handle, it sinks rapidly in nearly a vertical direction, and at the same time the pressure of the water throws up both the valves. In consequence of this, a current of water is constantly passing through the case while the instrument continues to sink. As soon, however, as by the check of the cord the instrument becomes stationary, the valves fall into their places, and intercept the escape of the water; the quicker the cord is drawn up, the more completely are the valves secured, and the influx of the upper water the more perfectly prevented. As soon as the case comes to hand, the lid is to be thrown back and the thermometer withdrawn just sufficiently to enable the observer to read off the degree at which the mercury stands.

44, Adelphi-street, Hutcheson Town, Glasgow, Feb. 25, 1820.

Dear Sir,—As I understand you have advised Mr. Jamieson, mathematical instrument maker of this city, to submit the model of the Marine Thermometer Case constructed by him, for my use, to the notice of the Society for the Encouragement of Arts, &c., and that you have expressed a wish to have a written statement of the causes which led to my application to Mr. Jamieson, in consequence of which he was led to undertake the construction of that instrument, and also of my opinion of its utility for nautical purposes, I shall endeavour to comply with your wishes as concisely as in my power; though I am much afraid the detail into which I must necessarily enter will not admit of all the brevity which might be desirable.

My attention was first directed to the thermometer, as a valuable nautical instrument, by the excellent memoir compiled by Mr. John Purdy, hydrographer, London, to accompany the admirable Chart of the North Atlantic Ocean, constructed by him, and published by Messrs. Whittle and Laurie, of 53, Fleet-street, in the year 1812.

The late celebrated Dr. Benjamin Franklin was, I believe, the first person who suggested the utility of the thermometer as an indicator of the *proximity of*, or an *approach to*, the American coast, having remarked the great difference of temperature between the water of the Gulf stream as it sweeps along the coasts of the United States, and the water upon the soundings between the inner edge of the stream and the shore.

This interesting subject was afterwards taken up by Colonel Jonathan Williams, who endeavoured, with some success, to attract the attention of nautical men to the vast importance of the thermometer, in an ingenious work intituled "Thermometrical Navigation," published at Philadelphia in 1799.

To avoid unnecessarily extending the length of this letter, by quoting evidence already before the public, I beg to refer to the third edition of the Memoir by Mr. Purdy, already mentioned, the perusal of which must satisfy the most sceptical, that the thermometer is not only a certain indicator of the proximity of the American coast, but also an infallible monitor of an approach to islands of ice, so perilous to navigators, and the dangers of which are tenfold increased by the circumstance of their being generally surrounded by dense fogs. Mr. P.'s Memoir, from the 73rd to the 78th page, is occupied with these important subjects.

Colonel Williams recommends that the thermometer should be slung, "so as to tow in the dead water of the ship's wake." I tried this mode, and had three of my thermometers broken in consequence: *this* led me first to think of having a case to prevent my instruments from being fractured by any accidental contact

tact with the ship. Some gentlemen, whose opinions I respected, suggested that while towed by the ship, her way through the water would naturally cause the thermometer to rise to the surface, and that thus only the temperature of the surface water could be obtained, and that they conceived this temperature liable to be influenced by the effects of the solar rays. To obviate this objection, I saw the necessity of a case to inclose the thermometer, which would admit the water freely as long as it was permitted to descend, but would shut as soon as it was begun to be hauled up.

After I was satisfied how useful a thermometer case, adapted properly for these purposes, must prove, I applied to various mechanics in different places to construct one for me; but they either did not sufficiently understand the matter, or considered the safety of human lives and merchant ships and cargoes of too trivial importance to induce them to exercise their abilities. Be this as it may, I was always disappointed of my object, until I became accidentally acquainted with Mr. Jamieson, who readily entered into my views, and made a case for my thermometer, which completely answers my ideas, of which the model intended to be sent to the Honourable the Society of Arts, &c. is, as you know, a correct copy, *i. e.* allowing for the difference of sizes, the original case being $1\frac{4}{10}$ inch in diameter within, which I consider large enough to contain a column of water that will retain its original temperature a sufficient space of time (after the case is hauled up) to allow of the degrees indicated by the scale being read off by the observer, which is all that is necessary; while, if the magnitude of the apparatus had been increased, it must have become almost useless from its weight, when a vessel had fresh way through the water, and by the time she came to run eight or nine knots, must have been totally unserviceable. The same objection applies to any addition of weight.

The objection to Jamieson's Marine Thermometer Case, which has, I understand, been made, that the valves do not permit a sufficient column of water to pass completely to fill the interior of the case in a constant stream as it descends, appears to me of little importance, as enough must pass through to regulate the altitude of the mercury in the thermometer tube; and before there can possibly be time to haul up any part of the attached line, the mere pressure of the water must infallibly fill the thermometer case.

Purdy's Memoir shows incontestably, that no vessel on board of which there is a thermometer, can possibly run ashore on the coasts of the United States of America to the northward of the Strait of Florida, without her commander (unless he is guilty of the most culpable negligence) having at least warning in sufficient

time

time to avoid the danger (*i.e.* if his vessel is not so much crippled as to render it impossible for him to use any means to get off shore); and various circumstances induce me to hope that the thermometer will ultimately be found not only an indicator of an approach to the coasts of the United States, but also, that it will point out the proximity of land or soundings in *all* places to the northward of the Tropics, and probably also to the southward of them, though my own experience does not warrant me in the hope that it will within them.

Besides these advantages, the use of the thermometer has already been ascertained as illustrative of several of the oceanic currents. The venerable and indefatigable geographer, Major Rennell, in a letter to myself dated 6th of November last, observes, " *The current from the Indian Ocean round the Cape of Good Hope differs* 10° *from the Ocean water*, i.e. *is* WARMER; *the* EQUATORIAL CURRENT *colder by* 5 *or* 6 *deg. than the Guinea current, which* BRUSHES *it in passing, &c.*" Thus far already established. We may surely venture to hope that as thermometrical observations are multiplied, their utility will become more obvious, and be more generally acknowledged.

In my own experience I have found a difference of 12 deg. in the temperature of the water in a few hours. Running out of the Delaware, in 9 fathoms water, the mercury stood at 60 deg. (in October 1817); as the water deepened, it rose to 64 deg.; and as we entered into the Gulf stream, it suddenly increased to 72 deg.

In September 1818, when bound from New Orleans to Gibraltar, in the ship Asia, of Scarborough, then under my command, a fever broke out on board the ship, and for a considerable number of days after we cleared the Strait of Florida, we had only four men and a boy fit for duty, and three out of that number merely convalescents. I was myself confined to bed, and my mate in the same situation; we were the only two navigators on board, and both unable to make up any reckoning, and some days unable even to crawl on deck to take an observation at noon. In this dilemma I trusted entirely to my thermometers, having given orders that the instant the mercury fell two or three degrees, the ship's head should be wore round off shore. In this way I kept the ship from danger, and also availed myself of the Gulf stream current to carry us to the northward; and I really believe that, under Divine Providence, the safety of the ship, cargo, and crew was attributable to my thermometers.

On the same voyage, on approaching very near the Azores Islands, the mercury sank 2 deg., and when we made the coast of Portugal, it rapidly fell from 69 deg. to 61 deg., at which it stood when we rounded Cape St. Vincent, at the distance of about

about a league. After we passed that promontory, the mercury again rose to 69 deg., at which it continued until the ship entered the Strait of Gibraltar. In the Strait the wind was adverse; and, in beating through it, the mercury stood at 68 deg. in the middle of the Strait, 64 deg. on the Spanish shore, and 61 deg. on the African coast: this difference between the temperature on the Spanish and African shores is apparently easily accounted for, as we stood much closer to the latter, avoiding the former on account of the rocks and shoals to the westward of Tariffa; yet as we found the mercury at our anchorage in Gibraltar Bay stood at 64 deg., perhaps there really was a difference of temperature in the water on the opposite shores: this was in October, 1818. Major Rennell, to whom science is under so many obligations, has communicated to me, that Captain Beaufort, of the Royal Navy, found a difference of 10 deg. between the water in the middle of the Strait and that in Gibraltar Bay, but I am not aware at what season Captain Beaufort's experiments were made.

Since Mr. Jamieson completed the thermometer case for me I carried it down to Greenock, and exhibited it to Mr. Colin Lamont, Mr. Quintin Leitch, and Mr. William Heron. Mr. Leitch procured a boat, and we went off, and made a number of experiments, which proved completely satisfactory.

My opinion of Jamieson's Marine Thermometer Case is, that the simplicity of its construction, and its strength, render it as complete a thing as could be desired for the purpose for which it is intended. I fully concur in the opinion expressed in the certificate by Messrs. Lamont, Leitch, and Heron; and I am decidedly of opinion that either to increase the size, or to add lead to it to increase the weight, will never be proposed by any *practical* seaman.

The mode in which I propose to use the apparatus is, by making about 20 fathoms of strong line fast to it,—to cause the officer of the watch to stand aft with the end of the line made secure to one of the staunchions, or some other thing, lest it should accidentally slip through his hands, and the whole be lost: he must then cause the apparatus to be passed forward, as in heaving the deep sea lead: when the vessel has much way through the water, it may be passed to the bowsprit end, and dropped (not hove) from it; but for general use the fore-chains may be far enough: when all is ready, let the officer call out to *let go*, and when that is done he must immediately haul in the line, and when he gets hold of the case, open it, and draw out the thermometer a sufficient length to read off the altitude of the mercury indicated by the scale, which he ought immediately to

mark on the log slate, in a column which should be traced upon the slate for the purpose.

In my own practice I have always caused two columns to be marked, the one T. A. for temperature of the air, the other T. W. for temperature of the water. In these columns I ordered the officer of the watch to insert the temperature both of the air and water twice (sometimes oftener) each watch. The temperature of the air is, however, of little importance further than as a comparative observation, it being only the variations of temperature in the water which are of importance to the navigator.

In reading off, the observer should take care to keep as much of the thermometer as possible immersed in the water, which this marine case allows of being done with facility.

I have said enough to convince you of the immense importance of the thermometer as a nautical instrument, and that Mr. Jamieson's Marine Case renders the use of it both more easy and certain. I am, Sir, ANDREW LIVINGSTON,
William Warren, Esq. Shipmaster.

P. S. You are at perfect liberty to use this letter in any mode you think may be advantageous to Mr. Jamieson, whom I consider entitled to much credit for this simple and useful invention.
A. L.

2nd P. S. Since writing the above, Mr. Jamieson has informed me that he is anxious to send the instrument with which the experiments were actually made at Greenock, and begged me to let him have it, promising to replace it to me. Although with some reluctance, I have complied. Thus the Society of Arts will actually have the real marine thermometer case in place of a model; and I doubt not but the scientific members of that honourable body, will neither wish its size or weight increased, whatever individuals (here) ignorant of seamanship may say. A. L.

XLIX. *Description of a Mercurial Log-Glass, invented by Mr.* C. H. JENNINGS, *of Carburton-street, Fitzroy-square**.

25, Carburton-street, Fitzroy-square, April 4, 1820.

SIR,—I HAVE the honour to send for the inspection and approbation of the Society, a Log-glass invented by me: it is intended to correct the errors constantly attending those at present used; the particles of sand with which they are generally filled being liable to cohere, particularly in damp weather, and thus to impede and even stop the performance of the instrument.

* From the *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1820. The Society's large Silver Medal was voted for this communication.

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The present log-glass runs so as to answer for both the long and short glass; the former is used when the ship's rate of going is slow, the latter when she has great velocity. It is intended to be exhausted of air, so that no oxidation of the mercury can take place, nor will it be influenced by the expansion, &c. of the air, which is the case with the sand glasses. The fluid mercury runs equally and more correct than sand, which always forms a hollow cone as it is running.

Those glasses have been much approved by nautical men; they are manufactured by Messrs. W. and T. Gilbert, mathematical instrument makers to the Honourable East India Company, Leadenhall-street.

Should this instrument deserve attention, I beg it may be placed among those valuable instruments and machines in your repository, which are so honourable to this country and to the Society. I am, Sir, &c.

A. Aikin, Esq. Sec. &c. H. C. JENNINGS.

Reference to the Engraving of Mr. H. C. JENNINGS's *Log-Glass,* Plate II.

Fig. 1, a section, *a a* the iron collar into which the two glasses *b b* are cemented; *c d*, two steel pipes fitted in the iron collar, so long as always to be above the surface of the mercury, and the glasses *b b* are so wide as to hold the mercury clear of the pipes in all positions: *e* the mercury, which is now running through the smallest aperture *c*, in 28 seconds (as marked on the glass, fig. 2); when inverted, the aperture *d* allows it to run in 14 seconds: *f*, a steel screwed mouth-piece to introduce the mercury, which may then be cemented up, or a valve may be applied and the glasses exhausted of air: *g* a steel cap to screw on to keep all safe. The glasses are secured by a horizontal circular piece of wood at top and at bottom, which are kept in their places by three screws *i i* which pass through the tubes or hollow pillars *k k*, one of which is shown in section: *l l* the screw nuts.

Fig. 2 shows the whole together, 14 and 28 being marked on the glasses: they are represented of one-third the real size.

L. *Notices respecting New Books.*

Lately published.

THE Grecian, Roman, and Gothic Architecture, considered as applicable to public and private Buildings in this Country. By W. FOX. 5*s*.

Index

Index *Monasticus;* or the Abbeys and other Monasteries, Alien Priories, Friaries, &c., formerly established in the Diocese of Norwich and the ancient Kingdom of East Anglia. By Richard Taylor, of Norwich. Folio. 3*l.* 3*s.*

Views of the Remains of Ancient Buildings in Rome and its Vicinity, with a descriptive and historical Account of each Subject.—By M. Dubourg. Atlas 4to. 26 plates coloured to imitate drawings. 7*l.* 7*s.*

Rome in the 19th Century; containing a complete account of the ruins of that ancient city, the remains of the middle ages, and the monuments of modern times. 8 vols. post 8vo. 1*l.* 7*s.*

Dr. Brewster's new Edition of Ferguson's Astronomy. 2 vols. 8vo. plates. 24*s.*

A Moveable Planisphere; exhibiting the face of the heavens for any given hour of the day, throughout the year, as also the time of rising and setting of the stars; designed to assist the young student in acquiring a knowledge of the relative situations and names of the constellations. By Francis Wollaston, F.R.S. 12*s.*

An Essay on Soils and Composts, and the Propagation and Culture of ornamental Trees, Shrubs, Plants, and Flowers. By T. Haynes, nurseryman, Oundle, Northamptonshire. 12mo. 5*s.*

Hints on an improved Method of Building, applicable to general Purposes. By T. D. W. Dearn. 8vo. plates. 4*s.* 6*d.*

Practical Observations on Midwifery, Part I. By John Ramsbottom, M.D.

Twelve Plates of Birds, designed for the use of the artist, connoisseur, and the naturalist. Demy folio. 5*s.*

A Manual of Lithography; or Memoir on the Lithographic Experiments made in Paris at the Royal School of the Roads and Bridges. Explaining the whole art, &c. Translated from the French by C. Hullmandel. 8vo. 6*s.*

Illustrations of British Ornithology, No. I. Elephant folio. 1*l.* 11*s.* 6*d.*, coloured 5*l.* 5*s.*

Annals of Oriental Literature. Parts 1, 2, and 3. By P. J. Selby. 8vo. 6*s.* each.

A Geographical Exercise Book. By C. Robertson, Surrey House Academy, Kennington Cross. 3*s.* 6*d.*

The Prize Essays and Transactions of the Highland Society of Scotland. Vol. 5. 8vo. 15*s.*

Horticultural Transactions. Vol. 4. Part II. 1*l.* 10*s.*

A Practical Treatise on the Inflammatory, Organic, and Sympathetic Diseases of the Heart; also on Malformations of the Heart, Aneurism of the Aorta, Pulsation in Epigastrio, &c.—By Henry Reeder, M.D. Member of the Royal Medical Society of Edinburgh, and of the Medical and Chirurgical Society of London.

A Nar-

A Narrative of Travels from Tripoli to Mourzouk, the Capital of Fezzan, and thence to the southern Extremity of that Kingdom.—By George F. Lyon, Captain R.N. 4to. with a map and 17 coloured plates, 3*l.* 3*s.*

Preparing for Publication.

Mr. John Farey, jun. is preparing a work on steam engines, illustrated wlth numerous engravings by Lowry.

Mr. Partington has also in readiness a work on steam engines.

A History of Shrewsbury. By the Rev. Hugh Owen and the Rev. J. Blakeway. 2 vols. 4to.

The concluding volume (being the 6th) of Dr. E. Clarke's Northern Travels through Denmark, Sweden, Lapland, Finland, Norway, and Russia.

An Historical and Topographical Account of Devonshire, being the 9th part of Magna Britannia. By the Rev. Daniel Lysons.

A Treatise on Indigestion.—By A. P. W. Philip, M.D.

Transactions of the Cambridge Philosophical Society. Vol. I.

M. de Humboldt's personal Narrative of his Travels to the Equinoctial Regions of the New Continent. Translated by Helen Maria Williams. Vol. 5.

The History and Antiquities of the Tower of London, with biographical Anecdotes of royal and distinguished Persons. By John Bayley, Esq. F.S.A.; with numerous engravings.

An Elementary Treatise on the Theory of Equations of the Higher Orders; and on the Summation and Reversion of Algebraic Series. By the Rev. B. Bridge.

Observations on the different Species of Inflammation. By J. H. James, Surgeon to the Devon and Exeter Hospital.

An Essay on Resuscitation. By T. J. Armiger.

Recollections of a Classical Tour made during the Years 1818 and 1819, in different Parts of Turkey, Greece, and Italy. By P. E. Laurent, Esq. Professor and Teacher of Languages at Oxford. With costumes. 4to.

A Treatise on the Epidemic Cholera of India. By James Boyle, Surgeon of His Majesty's ship Minden.

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 21.—A PAPER by Dr. Henry of Manchester, on the Aeriform Compounds of Charcoal and Hydrogen, which had been partly read at the two preceding meetings, was concluded. The first object proposed by Dr. Henry was, to examine how far those

those views of the compounds of charcoal and hydrogen which had arisen out of his former experiments, and those of Mr. Dalton, were correct; especially whether there be a compound answering in its characters to light carburetted hydrogen gas, the existence of which had been called in question in a late Bakerian lecture. This, after attentively, and at various times, examining the gas from stagnant water, he pronounces to be a distinct chemical compound, having uniformly the same composition and chemical properties, and the same specific gravity (0·556). It is constituted of 100 parts by weight of charcoal united with 33·40 of hydrogen; while olefiant gas consists of 100 charcoal + 16·70 hydrogen. Hence, if the latter be considered as a compound of one atom of charcoal and one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal and two atoms of hydrogen; and as 100 cubic inches of carburetted hydrogen contain hydrogen equivalent to 200 cubic inches of hydrogen gas, he suggests the verification of the specific gravity of hydrogen gas by that of carburetted hydrogen, and finds that in this way it comes out 0·0698, making the relative weights of the atoms of hydrogen and oxygen very nearly as 1 to 8. The atom of charcoal also he estimates from the composition of carburetted hydrogen, and of carbonic acid, at 6.

His next experiments relate to the best means of analysing mixtures of olefiant gas with hydrogen, carburetted hydrogen, or carbonic oxide; and of olefiant gas with all those three gases. Chlorine, he shows, may be employed with perfect accuracy, provided certain precautions are observed, which are described at length in the paper. The chief of these is the complete exclusion of light, for in that case olefiant gas alone is condensed; but even the faint light of a cloudy day was found sufficient to cause the speedy action of chlorine on the other gases. The paper contains also directions for analysing mixtures of hydrogen, carburetted hydrogen, and carbonic oxide; but these, from their nature, are incapable of abridgment.

By the analytical processes thus established, he proceeds to examine the composition of oil gas and coal gas. The results are given in tables; but the general issue of the experiments is, that oil gas (as he had formerly shown with respect to coal gas) is very far from being uniform in composition, but differs greatly in specific gravity and combustibility, when prepared at different times even from the same kind of oil, owing to variations of temperature and other circumstances. Essentially the gases from oil and from coal are composed of the same ingredients, though in different proportions, viz. simple hydrogen, light carburetted hydrogen, and carbonic oxide gases, with the addition of variable proportions of an elastic fluid, which agrees with olefiant

fiant gas in being condensable by chlorine, but consumes more oxygen and gives more carbonic acid, by combustion, and has a higher specific gravity than olefiant gas, and even than atmospheric air. Whether this ingredient be strictly a gas, permanent at all temperatures, or a mixture of olefiant gas with some new gas, constituted of hydrogen and charcoal in different proportions from what are found in the known compounds of those elements, or merely the vapour of a volatile oil, he leaves to be decided by future experiments.

March 8.—At this meeting was read a paper on the length of the seconds pendulum in different latitudes.—By Capt. Sabine.

15.—Observations on Naphthalin.—By Dr. Kid.

21.—A paper by Mr. Herschel, on the aberration of compound lenses; also

A paper on the skeleton of the Dugong.—By Sir E. Home; and

A paper by Sir Humphry Davy, on the Papyri of Herculaneum.

ASTRONOMICAL SOCIETY OF LONDON.

April 13.—A letter was read from M. Nicollet of Paris, communicating the elements of the late comet, as deduced from the observations of MM. Bouvard, Arago, and himself, from Jan. 21 to March 1. And he adds, that on account of the small perihelion distance, the motion of the comet will accelerate, and soon *become visible again* in the morning before sunrise.

A letter was also read from Dr. Olbers of Bremen, respecting the same comet; giving the elements as deduced from his own observations, and from the observations of Professors Nicolai of Manheim, Encke of Seeberg, and Staudt of Gottingen. Dr. Olbers likewise states that he observed the remarkable luminous appearance on the dark limb of the moon on the 5th of February last, as mentioned by Capt. Kater in his paper read before the Royal Society. But he adds, that he cannot yet bring himself to believe in the existence of any fiery volcano in the moon, and thinks that the appearance is to be satisfactorily accounted for in another manner, and more consistent with what we know of the physical construction of the moon. The luminous appearance seemed to be situated in or near the spot called Aristarchus, with the general appearance of which Dr. Olbers was perfectly acquainted. But this phænomenon presented quite a different aspect.

A paper was next read—"On the description of a repeating instrument upon a new construction; by G. Dollond, Esq.": wherein the author particularizes several improvements adopted in the construction of this instrument; and which promise to be

 not

not only of considerable advantage to science, but also a great convenience to the observer.

FRENCH ROYAL ACADEMY OF SCIENCES.

The following prize subject has been announced by this Academy for competition during the years 1821 and 1822:

"To trace the gradual development of the aquatic Triton or Salamander through its different degrees, from the egg to the perfect animal, and to describe the internal changes which it experiences, but principally in regard to osteogony and the distribution of the vessels."

Prize—a gold medal of the value of 300 fr. to be adjudged in the month of March 1822; essays to be sent in by the 1st of January 1822.

ROYAL ACADEMY OF SCIENCES OF BERLIN.

The following question has been announced by this Society for competition during the year 1822:

"Among the luminous circles which appear in the heavens when the atmosphere is not entirely serene, the rainbow alone is explained in a satisfactory manner, inasmuch as the phenomena which it presents depend simply on the laws of refraction and reflection of the light: but with respect to those luminous crowns which we see frequently around stars, an explanation is wanted more complete, and developed with more precision than any which has been yet given. Not wishing to refer the phenomena of crowns and parhelions to any thing else than reflexion and refraction, and to discover in consequence the form, the position, and the interior structure, proper for the small corpuscula supposed to exist in the atmosphere, the efforts of philosophers have not been either very happy or very conformable to nature. It is probably to other properties of the light that the phenomena of these rings ought to be ascribed; properties which are subject to geometric laws, and may be manifested by philosophical experiment. The recent researches upon light have thrown so great clearness on several suspected or neglected theories, and have so advanced a knowledge of its modifications, that it may be possible even to look upon the problem of the rings as no longer unsusceptible of a new solution. However, the results hitherto obtained appear incomplete and not sufficiently exact; and being as yet but slightly compared with the phenomena, it is desirable to see a more delicate theory developed, and its agreement with the truth better established by multiplied and circumstantial observations. The Society proposes therefore as a subject of prize—

"To give such a mathematical and detailed explanation of the luminous and coloured rings which are observed round the

the moon and the sun, as may be conformable to the experiments made on light and the constitution of the atmosphere, and also in accordance with observations as precise as the nature of the phenomena will admit."

Memoirs to be given in before the 31st March 1822.—Prize 50 ducats.

HELVETIC SOCIETY OF NATURAL SCIENCE.

This Society has proposed the following prize question for the years 1822 and 1823:

"To collect exact and well-observed facts on the increase and diminution of the glaciers in the different parts of the Alps, and on the deterioration or amelioration of their pasturages, and on the former and present state of the forests." It will be sufficient if the authors treat only of a determined part of the Alps.—Memoirs to be given in before 1st of January 1822.—Prize 300 livres.

SOCIETY OF SCIENCES AND ARTS OF UTRECHT.

This Society has announced the following questions for prize competition:

First Question.—"Are there characteristic signs sufficient to distinguish always with certainty the true cancer from other maladies which resemble it? In case of an affirmative answer, what are these signs? Ought this malady to be considered as the effect of an indisposition of the whole body, or as only local? If it is to be considered as an indisposition of the whole body, can external remedies, whether amputation or the remedy applied by the Religious of the Convent of Rees, or the corrosive remedies, especially arsenic, contribute to the cure or alleviation of the malady? or ought they to be considered as all equally hurtful? When the malady has not yet the characteristic signs of true cancer, but when there is reason to fear it may become so, and when it may as yet be considered as a local evil, what external remedies may then be applied with sound hope of success? and what are those which should be considered as hurtful?"

Second Question.—"Can we, by surveying any particular part of the body of an animal that we have not had an opportunity of observing in life, conclude with certainty what use it made of that part; so that we may look on this principle of final causes not only as an useful principle, but as always a sure guide in the natural history of the animal kingdom?"

Third Question.—"What relation is there between speculative philosophy and mathematics? Why are mathematics necessary to philosophy, excluding their application to physics? and what means does speculative philosophy offer for the extensive and ultimate perfection of pure mathematics?"

LII. *Intelligence and Miscellaneous Articles.*

NEW ALKALIES.

1. *Daturium.*—M. Brandes has given the name *Daturium* to a substance supposed to be an alkali obtained by him from the seeds of the *Daturium Stramonium.* It is combined in the seeds with malic acid, and is separated in the usual manner. In water and in cold alcohol it is nearly insoluble, but is soluble in hot alcohol, from which it precipitates in flocculi on cooling. It crystallizes with difficulty in quadrangular needles. It forms neutral salts with acids, but requires to be added in great quantity. Its sulphate is crystallizable, soluble in water, efflorescent, and decomposed by fixed alkalies: the muriate forms square plates easily soluble in water: the nitrate is crystalline and soluble: the acetate deliquescent. It acts on iodine in the manner of other alkalies, but freely.—*Journal de Physique,* xci. p. 144.

2. *Atropia.*—This name is given to an alkali, found by M. Brandes in the *Belladonna Atropia,* which owes its peculiar properties to this substance. It is white, shining, crystallizes in long needles, insipid, and little soluble in water or in alcohol; forms regular salts with the acids, and neutralizes a considerable quantity of them. The sulphate of *Atropia* contains

Atropia	38·93
Sulphuric acid ..	36·52
Water	24·55
	100·00

When mixed with potassa and raised to a red heat, if mingled with muriate of iron it produces a brilliant red colour.

3. *Hyoscyama* is extracted from the *Hyoscyamus niger,* and is not easily altered even by a red heat. It crystallizes in long prisms, and forms very characteristic salts when saturated with sulphuric or with nitric acid.

Great caution is required in examining the constituent alkaline principles of narcotic plants, as their poisonous properties reside concentrated in the alkaline substance. The vapour is very injurious to the eyes, and the most minute fragment placed on the tongue is highly dangerous.—*Journal de Physique,* xci. p. 239.

STEAM-ENGINES AT CONSOLIDATED MINES *.

At the latter end of December last, the third of the new steam

* Annals of Philosophy, No. 3.

engines

engines erected on the Consolidated Mines near Redruth, in Cornwall, was put to work; and as two of these machines are of a larger size than was ever before attempted, and as the concern is one of great extent and interest, some account of it may be acceptable to our readers.

The undertaking includes four or five copper mines nearly adjoining, and on the same veins, formerly worked very profitably in distinct portions, until owing to difficulties in pumping the water, and other circumstances, they were gradually abandoned about 16 years since.

The improvement in the use of steam power since that period is a prominent reason for expecting advantage to those who have had the spirit to renew the workings, though there are many others which are important, such as increased skill in the management of mining processes, and a reduction in the cost of labour and those materials which are most largely consumed. The present company have engaged a capital of about 65,000*l.* in the concern, and the whole is under the management of Capt. William Davey and Mr. John Taylor.

The extent of underground workings to be drained is very considerable, running for about a mile in length, and reaching at the lowest part to a depth of about 130 fathoms under the adit, or level by which the water is discharged towards the sea.

To keep the whole of these excavations dry, and to enable the mines to be sunk deeper, three engines have been erected by Mr. Arthur Woolf. One at the western extremity of the ground having a cylinder of 70 inches diameter, which works a pump about 60 fathoms deep, and two others, which we mean particularly to notice, are situated one near the centre, and the other at the eastern end of the concern.

These engines have cylinders of 90 inches diameter, the pistons make a stroke of 10 feet in the cylinder, and the centre of the beams is so fixed that the rods make an eight foot stroke in the pumps; thus they are able, at the common pressure, to raise a load of 85,000 lbs. Each of the engines is furnished with six wrought-iron boilers for producing high pressure steam, which is applied in the mode usually called expansive by engineers, and is condensed in the common manner.

Three boilers are connected so as to be heated by two fires, and are sufficient to work the engine, leaving three others to be applied when those which have been in use are cleansing or repairing.

These immense engines are executed in a very beautiful manner, and exhibit remarkable instances of accurate workmanship and sound calculation. Though they exceed in power all others that have

have been before constructed, and of course every part is of a dimension for which there is no precedent, yet each has from the first performed its office aright, and the combination is so perfect that the motions are equable, and free from jar or concussion. The engines have worked repeatedly for days at the rate of twelve and thirteen strokes a minute, and the whole has gone as smoothly as if a fly wheel regulated the impulse.

The effect of the first of these engines, or, as it is called in Cornwall, the duty, has been regularly calculated by the person appointed for that purpose, and has been published in the monthly report.

It was found to have consumed about 3,800 bushels of coal in 35 days, or 111 bushels per day; and the effect had been that of raising 38,500,000 lbs. of water by each bushel of coal, which is rather more than was done in the same period by any engine of similar construction.

It may be worth mentioning the weights of some of the principal parts of one of these large machines. The cylinder, exclusive of the cover and bottom, weighs about 12½ tons, in one piece; it is surrounded by a case of still greater dimensions. The beam with its gudgeon weighs nearly 25 tons.

The pump rods in the shaft are the largest mast timber that could be procured, and are 16 inches square to a considerable depth in the shaft: when the whole are attached, they will weigh, with the iron plates which connect them together, nearly 40 tons.

When it is considered that to this latter weight is to be added that of the column of water, and one half of the beam, we shall find nearly 100 tons on one side the centre, and of course a corresponding pressure on the other side to counterpoise it; so that there is suspended on the gudgeon, and moving freely upon it, nearly 200 tons.

The piston frequently passes through 240 feet every minute, and gives a corresponding velocity of motion to this immense mass of matter, which is yet regulated with a precision that is astonishing, and which acts without concussion, and without disturbance to the various parts of the machine.

There are many most ingenious improvements in the construction, and the arrangement is simple and complete. The whole reflects great credit on the skill and ability of Mr. Woolf, to whom Cornwall has before been indebted for the introduction of some of the most important improvements in steam engines that have benefited the mines in later years.

The works at the Consolidated Mines were only commenced in January 1819, and it is probable that in a few weeks the water will be all pumped out; so that this, with the extensive erections

tions for various purposes on the surface, which are such as to render it probably the largest and most complete mining establishment in the world, will have been executed in two years.

INDIAN CORN.

Dr. John Gorham, of Harvard University, Cambridge (U. S.), has lately examined this grain, chemically, with great attention. His experiments were made upon two varieties of maize, that producing small yellow grain, and the large flat white kind, known by the name of Virginian corn. The results were so similar, that those only belonging to the former have been detailed. According to his analysis, the constituents of yellow Indian corn in the common and dry state are:

	Common State.	Dry State.
Water	9·0	
Starch	77·0	84·599
Zeine	3·0	3·296
Albumen	2·5	2·747
Gummy matter	1·75	1·922
Saccharine matter	1·45	1·593
Extractive matter	0·8	0·879
Cuticle and ligneous fibre	3·0	3·296
Phosp. carb. sul. of lime and loss	1·5	1·648
	100·	99·980

Zeine is a yellow substance resembling bees' wax, soft, ductile, tenacious, elastic, insipid, nearly inodorous, and heavier than water. When heated it swells, becomes brown, smells like burning bread, melts with the odour of animal matter, and leaves a bulky charcoal. It burns in the flame of a lamp, but not rapidly. Seems to yield no ammonia on distillation. It is insoluble in water—soluble in alcohol, oil of turpentine, and sulphuric ether, and sparingly so in mineral acids and caustic alkalies. It is insoluble in fixed oils, but mixed with resin. The quantity obtained from 100 grains was three grains. The substance to which Dr. Gorham has given the name of Zeine appears to differ from all known vegetable bodies: it resembles gluten in some circumstances, but differs from it in containing no azote, in its great solubility in alcohol, and in its permanency, not undergoing any obvious change in six weeks. On the other hand, it is analogous to the resins in its solubility in alcohol, essential oils, alkalies, and partial solubility in acids. It is inflammable, and probably composed of oxygen, hydrogen, and carbon. It may easily be obtained by digesting a few ounces of the meal from the yellow corn in a flask with warm alcohol, allowing it to rest for some hours, then filtering and evoporating.

LIST OF PATENTS FOR NEW INVENTIONS.

To Ilario Pellafinet, of Earl's Court, Middlesex, gentleman, for certain new and improved machinery and methods for breaking, bleaching, preparing, manufacturing and spinning into thread or yarn, flax, hemp, and other productions and substances of the like nature capable of being manufactured into thread or yarn.—Dated 27th March 1821.—12 months allowed to enrol specification.

To William Southwell, of Gresse-street, Rathbone-place, piano forte manufacturer, for improvements on cabinet piano fortes.—5th April.—2 months.

To James Goodman, of Northampton, sadler, for his improvement on stirrup irons.—5th April.—2 months.

To Lieut.-Colonel Henry Goldfinch, of Hythe, Kent, for his improvement in the formation of horse shoes.—5th April.—6 months.

To William Annesley, of Belfast, architect, for certain improvements in the construction of ships' boats and other vessels.—5th April.—2 months.

To William Chapman, of Newcastle-upon-Tyne, civil engineer, for his method of transferring the ladings of lighters and barges into ships or vessels, or from ships or vessels into lighters and barges. —April.—2 months.

To James Henry Marsh, of Chenies-street, Tottenham Court Road, for certain improvements on wheeled carriages.—17th April.—2 months.

To James Smith, of Hackney, gentleman, for improvements in the machinery employed for shearing or cropping woollen cloth.—18th April.—2 months.

MATHEMATICAL QUESTIONS.

To Mr. Tilloch.

Sir,—I. In contemplating the general figure of the curve called the *Lemniscate*, shown in the "Philosophical Transactions" for 1820, by *Mr. Herschel junior*, to be traced by the Tints of certain polarized Rays of Light, it appears that in one certain proportion of *a* to *b* (see his Equation) the Curve approaches the nearest in form and dimensions to an Apolonian Ellipsis; it is required to assign this proportion?; and show in such case, how far the *focii* and axes of the Lemniscate are, from coinciding with those of the Ellipsis?

II. It also appears, that in one certain proportion of the above Elements, there is *a straight part* of the Curve; it is required to assign these proportions?, and the length and position of such straight part?.

III. To show how the method of investigation in the last question,

tion, may be applied to define the *straight parts* of other Curves?; that for instance, so very ingeniously and usefully produced, by the combination of Levers which direct the Piston-rod of Messrs. Watt and Bolton's Steam-Engines?

IV. Mr. Adam Anderson in the last Number of the Edinburgh Philosophical Journal, has investigated Rules, for calculating *the Dip*, or visible depression of the Horizon at Sea: and (allowing for Refraction, at the rate of ·08 of the intercepted Arc of a great Circle of the Earth) *finds this dip in Minutes, to be very nearly equal to the Square-root of the Height in Feet, of the Observer's Eye above the Sea:* he also mentions, as the usual approximate Rule for the converse of this in the practice of *Levelling* on Land, the *taking* $\frac{2}{3}$rds *of the Square of the Distance in* (English) *Miles, as equal to the correction in Height, for the Earth's curvature.* Quere, Is the last of these Rules consistent with the first?; also, What are the most simple and consistent decimal Multipliers for the *Square-root* in the first Rule and for *Square* in the second Rule, to be sufficiently correct in practice?!! and What are the Heights and Corrections, to which such Rules will apply, without sensible errors? I am, yours, &c.

London, April 9, 1821. An Engineer.

CHRONOMETERS.

'Change Alley, March 30, 1821.

Sir,—In consequence of a report injurious to our reputation, namely, that a Mr. Molyneux (and not ourselves) was the maker of the chronometers which we had the honour of sending out with Capt. Parry, and being apprehensive that this report may have reached you, we trust to your goodness to excuse our thus troubling you with the following statement, as we are particularly desirous that you should be rightly informed on the subject.

We beg positively to state, that Mr. Molyneux did not make any part of those chronometers, nor were they ever in his possession, neither did he ever see them. He has not been employed by us for about these three years past until the present month. The whole of the chronometrical parts were made in our house under our direction, by workmen articled to us for the purpose of instructing them in that branch of the business; and the final corrections and adjustments were completed by ourselves personally, in a method peculiarly our own, from a conviction that we had discovered the causes of the material alterations which chronometers frequently make in their rates. This method of correction was applied, to prevent that alteration, the particulars of which we stated in a letter to Capt. Sabine at the time the chronometers were delivered to him for trial. That we have succeeded in accomplishing this, is not only proved by the Polar chronometers, but by others, we corrected at the same time, and which

which have gone equally well. We cannot offer a greater proof that we are practical artizans, than by stating, that we are employed by some of the trade to make their chronometers entirely complete and ready for sale; and we have sold to different watchmakers twenty marine chronometers in one year, a number that no private man could possibly make himself without employing other workmen:—but still we trust he would be considered the maker of them all, as being made under his direction, and he of course being responsible for their performance. We have for the last five years made on an average, forty marine chronometers per year, and our demand is considerably on the increase. With the exception of eight or twelve, the whole of our chronometers for three years past have been made in our own house, many of them sprung, and the whole without any one exception adjusted in temperature, and finally completed and put together by ourselves personally. About five years ago, owing to a great demand for chronometers, we applied to Mr. Molyneux (who was employed with others at that time by us in making escapements) to assist us in springing. He recommended us to a person who he informed us sprung for him, as it interfered too much with the operative part of his business to do them himself. We sent some to Mr. Molyneux to get done for us, who afterwards told us that we had better send them to the person ourselves, as it would occasion less trouble. This workman has been employed both by Mr. Molyneux and ourselves ever since, to put chronometers in that state in which their more accurate adjustments may be perfected and completed by ourselves personally.

The most important feature in the Polar chronometers (and which we feel perfectly justified in saying was produced by their mode of correction) was, that they took up steady and uniform rates from the time they were put together, immediately after their corrections were completed, none of them having been going more than three weeks before they were delivered, and one of them not ten days.

In risking our property in the late hazardous enterprize, we were actuated only by those motives which should stimulate men in every pursuit through life, "a desire to excel in their profession;" and we cannot but feel mortified at any attempt to deprive us of the credit as the makers. The report existing in any other quarter would have been unworthy our notice; but finding it very generally circulated amongst gentlemen of science whose favourable opinion we are anxious to cultivate, we have been induced to make this statement. We have the honour to be, sir,

Your most obedient humble servants,

To Mr. Tilloch. PARKINSON and FRODSHAM.

P. S.

P. S.—Since we had the honour of drawing out the preceding statement for your inspection, Mr. Molyneux has declared to Mr. Frodsham, in the presence of a third person, "That he never did see the chronometers we sent out with Capt. Parry," but still considers himself the maker for the following reasons, viz. that a Mr. Hopkins had sprung them, who he said was his (Molyneux's) workman, and that he considered work done by the workman, done by the master. In this principle we agree, provided the work is done under the inspection of the master, but not otherwise. But we never did consider Mr. Hopkins in the exclusive employment of Mr. Molyneux; and even if he were, the person who springs a chronometer cannot be regarded as the maker, though it is an essential part of the work; for in this case no master would be entitled to credit for his chronometers, as it is the general practice to employ workmen to spring them; and Mr. Molyneux does the same thing. The practical masters (amongst whom we rank ourselves) finally adjust them, and without making their final adjustments we could not deliver them with confidence.

Mr. Hopkins receives only half the price for springing our chronometers, that he is paid by other watchmakers who depend entirely upon him to complete theirs.

BAROMETRICAL OBSERVATIONS.

Crumpsall, Lancashire, April 10, 1821.

SIR,—I send you observations made at this place on Monday the 9th instant.

Mr. Thomas Hanson, of Manchester, having favoured me with his observations of the same date, I transmit them to you with my own.

Mr. Hanson resides in Bridge-street, and his barometer, which like mine is a common upright one, is four feet from the pavement, and about eight feet higher than the bridge at the bottom of the street; which, as I have before stated, is exactly 28 feet above the level of the river Irwell.

I am, sir, your obedient servant,

To Mr. Tilloch. JOHN BLACKWALL.

CRUMPSALL.

1821. A.M.		Bar.	Ther. att.	Ther. det.	Wind.	Weather.
April 9th	8h.	29.640	52°	52°	W. by S. light	Sunshine with clou.
	9	29.640	53	53 5	W. by S. do.	Do.
	10	29.635	55	55 7	S. W. fresh.	Do.
	11	29.625	55·5	57	W. brisk.	Do.
	12	29.605	55·5	57 7	W. do.	Do

MANCHESTER.

1821. A.M. April 9th,	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
8h.	29.860	58	56	S.S.W. light.	Fine.
9	29.855	60	60	S.S.W. do.	Slightly cloudy.
10	29.847	61·5	64	W. do.	Cloudy.
11	29.844	62·5	64	W. brisk.	Cl. with gleams of sunshine.
12	29.815	64·5	63	W. do.	Slightly cloudy.

Epping, April 19, 1821.

Sir,—I again trouble you with some further barometrical observations, taken on the 12th of March and 9th of April, they are as follow:

1821. A.M.	Hour. M.T.	Barom.	Ther. att.	Ther. det.	Wind and Weather.	
Mar. 12th.	8h	29·597	48	41	W.S.W.	Thin and flying nascent clouds, little wind.
	9	29·600	48	42		
	10	29·607	47	43		
	11	29·607	47	45		
	12	29·607	47	48		
April 9th.	8h	29·700	54	50	N.W.	Very little wind, with clouds moving in different directions, sun at times.
	9	29·700	54	53	N.W.	
	10	29·700	54	55	N.W.	
	11	29·700	55	57	N.W.	
	12	29·678	55	58	W.S.W.	

I received the following correct observations, which were taken at Arundel, on April the 9th, by my very ingenious friend Mr. G. Constable.

		Barom.	Ther. attached.	Ther. detached.	Wind.
April 9th.	8h	30·101	55	54	W.
	9	·100	55	55	W. by N.
	10	·100	56	55	N.N.W.
	11	·096	57	57	N.N.E.
	12	·088	58	57	E. by S.

Mr. Constable moreover observes, "I cannot yet correctly tell the height of my situation above the level of the sea, but I intend levelling from the river which runs at the bottom of the town; the fall of water to the sea being about 2½ feet; or, if I can procure a good spirit level, I will level from the sea at low water, a distance of 4½ miles." From the proximity of Arundel to the sea,

sea, it must be considered as a valuable station for ascertaining the point of zero, if care is taken to obtain the exact altitude of the surface of the mercury in the basin of the barometer above the said point. Yours most respectfully,

To Mr. Tilloch. THOMAS SQUIRE.

Bristol, April 19, 1821.

SIR,—I send you the barometrical observations for March and April, taken at the times proposed by Mr. Bevan; and am, sir,

Your obedient servant,

EDWARD JONES.

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Weather.
Mar. 12th.					
8h A.M.	29·800	53	46	W. by S.	Fine.
9	29·800	54	48½	Do.	Do.
10	29·805	54½	51½	W.	Cloudy.
11	29·810	54	49	W. by N.	Rain.
12	29·820	56	55	Do.	Fine.
April 9th.					
8 A.M.	29·930	54	49	W. by N.	Hazy.
9	29·915	54½	51	W.	Do.
10	29·905	55½	54½	Do.	Fine.
11	29·890	57½	58	Do.	Do.
12	29·875	59	59	Do.	Do.

Leighton, April 25, 1821.

SIR,—Engagements that took me from home, have this month prevented me from sending the observations made at this place on the barometer, before the present late period in the month. If not too late, they are at your service.

April 9, 1821.	Barom.	Ther. att.	Ther. det.	Wind.		Weather.
8h	29·803	49¼	48	W.S.W.	light.	Misty.
9	29·800	50	52	W.	do.	Do.
10	29·796	51	54	S.W.	do.	Fine with clouds.
11	29·778	51	55	W.	do.	Few clouds.
12	29·768	51½	59	S.	do.	Do.
1	29·751	52	59	N.W.	do.	Do.

It is proper to observe, that the first observation was made a few minutes after eight; the remainder at the proper time.

Col.

Col. Beaufoy has also favoured me with the observations made at Bushy-Heath, as below:

1821.	Barom.	Ther. att.	Ther. att.	Wind.		Weather.
April 9. 8h	29·563	51	50	W N.W.	light.	Very fine.
9	29·561	51	53	S.S.W.	do.	Foggy.
10	29·554	51·7	55	S.S.W.	do.	Cloudy.
11	29·551	52	55	S.	do.	Do.
12	29·527	53·7	58	W.N.W.	do.	Fine.
1	29·517	55	59	W.	fresh.	Do.

From a comparison of the observations made at Leighton and Bushy, in March, it might be supposed that some addition had been made to the quantity of mercury in the basin of my barometer, but no such addition or any alteration was made.

B. B.

A Mean of Meteorological Observations for the last Seven Years.

	Mean of Thermometer.			Thermom.		Mean of Barometer.			Barometer.		Prevg. Winds No. days of each.				Variable.	Rain or Snow.	Fall of Rain.
	A.M. 8	P.M. 2	P.M 10	Max.	Min.	A.M 8	P.M. 2	P.M. 10	Max	Min.	N.	E.	S.	W			
1814	45·41	51·78	44·66	76	15	29·64	29·64	29·64	30·30	28·30	91	15	174	42	13	175	*
1815	47·45	53·90	46·46	30	17	29·60	29·60	29·62	30·40	28·55	73	29	183	69	11	185	29·36
1816	43·85	50·48	43·89	78	14	29·50	29·49	29·50	30·50	28·40	69	29	178	78	11	203	31·95
1817	45·13	51·36	45·69	81	18	29·56	29·56	29·56	30·35	28·25	83	31	168	64	18	188	32·01
1818	45·67	52·87	47·00	82	18	29·41	29·41	29·41	30·25	27·75	50	45	199	60	9	161	25·58
1819	45·40	52·31	46·27	80	1	29·38	29·38	29·38	30·05	28·45	90	41	166	60	8	189	31·62
1820	43·61	50·90	42·25	84	5	29·44	29·44	29·45	30·40	28·15	78	41	168	60	19	163	31·56

* Rain not ascertained in 1814.

The above observations were kept and communicated by the Rev. Edward Stanley, Rector of Alderley, Cheshire.

NAUTICAL ALMANAC.

It has been stated in the public papers, that an error of six minutes in the plane of the moon's node, pervades the Nautical Almanac for 1821, 1822, and 1823: and that the obliquity of the ecliptic is also incorrectly given. Should this be the case, we doubt not the Commissioners of Longitude will correct these errors in the subsequent numbers of that useful work.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE.

BY MR. SAMUEL VEALE.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
Mar. 15	11	49·5	30·15	Fine
16	12	51·	30·04	Ditto
17	13	51·5	29·70	Ditto
18	full	46·	29·20	Stormy
19	15	42·5	28·95	Ditto
20	16	46·	29·05	Ditto
21	17	46·5	29·20	Cloudy
22	18	45·	29·95	Ditto—squally with rain A.M.
23	19	43·	29·90	Ditto
24	20	47·	29·48	Stormy
25	21	51·5	29·20	Cloudy
26	22	51·5	29·40	Fine
27	23	44·5	29·03	Stormy
28	24	42·5	29·	Rain
29	25	48·	28·83	Cloudy—rain A.M.
30	26	51·5	29·50	Fine
31	27	45·	29·13	Rain
April 1	28	51·	29·43	Fine
2	new	56·5	28·95	Cloudy—rain A.M.—rain in the afternoon with sunshine which produced a brilliant rainbow.
3	1	52·	28·90	Fine
4	2	49·	29·10	Cloudy
5	3	41·	29·45	Rain
6	4	47·	29·90	Cloudy—rain A.M.
7	5	60·5	29·80	Ditto
8	6	59·5	29·90	Fine
9	7	62·	29·70	Ditto
10	8	58·	29·55	Cloudy
11	9	59·	29·35	Fine
12	10	48·	29·	Rain
13	11	52·	29·12	Fine—hail and rain P.M.
14	12	49·	29·30	Rain

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For April 1821.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
March 27	44	47	40	29·40	Showery
28	48	43	40	·14	Rain
29	40	46	38	·27	Rain
30	39	50	40	·76	Fair
31	40	47	38	·42	Stormy
April 1	36	51	43	·68	Fair
2	47	59	41	.87	Fair
3	42	51	40	·45	Stormy
4	42	52	41	·37	Fair
5	41	52	40	·67	Stormy
6	40	47	43	30·13	Cloudy
7	43	59	52	·12	Cloudy
8	50	66	50	·29	Fair
9	50	62	50	·03	Fair
10	50	58	53	·01	Fair
11	50	59	46	29·66	Fair
12	43	51	44	·37	Showery
13	42	52	41	·50	Fair
14	40	47	40	·51	Stormy
15	38	48	39	·57	Storms with hail
16	42	55	43	·49	Fair
17	41	55	45	·55	Showery
18	46	55	46	·78	Fair
19	47	52	46	·69	Small rain
20	50	59	50	·61	Do. with thund. in
21	50	58	49	·86	Cloudy [the eveng.
22	46	58	47	·98	Fair
23	50	67	52	·58	Cloudy
24	55	66	56	·56	Fair
25	55	73	59	·76	Fair
26	62	74	60	·80	Fair

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

9th April	8 o'Clock M.	Barom.	30·100	Ther. attached	52°	Detached 50
— —	9	— — —	30 094	— — —	52	— — 50
— —	12	— — —	30 064	— — —	57	— — 59
— —	1	— N. —	30 028	— — —	58	— — 62
— —	2	— —	30 028	— — —	59	— — 62

LIII. *On the atmospherical Refraction.* By Mr. J. Ivory.

To the Editor.

Sir, — In some researches on astronomical refraction I found the subjoined formula, which is not unworthy of the attention of astronomers on account of its exactness, its convenience for calculation, and because it comprehends all the refractions from the zenith to the horizon in one expression. It is not an empirical formula, or one obtained by altering the coefficients till they represent with sufficient accuracy a certain number of observed places. It is entirely theoretical, and, in fact, borrows nothing from astronomical observations. The only elements that enter into its construction are the numbers that denote the refractive force of the air; the mean pressure of the atmosphere; and the mean horizontal refraction; quantities which have been taken as they are laid down in the *Mécanique Céleste.* In its original state, the formula would therefore give the refractions for the mean height of the barometer adopted by the French astronomers, and for the temperature of melting ice; but, in order to facilitate a comparison with the Table published in the *Connaissance des Tems,* it has been reduced to the temperature of 10° of the centigrade thermometer, or 50° of Fahrenheit, by applying the correction directed to be used with the Table. It would occupy too many of your pages, sir, to enter on a detail of the analysis; but I shall attempt below to point out the principles on which the investigation proceeds. I put A for the zenith distance, and R for the refraction: then, having found the subsidiary angle ϕ by means of the formula,

$$\text{Log. tan } \phi = 18{\cdot}9873149 - \text{log. cos } A,$$

we have

Log. of the coefficients.

$$R = \sin A \times \left\{ \begin{array}{lll} 1200''{\cdot}93 \tan \tfrac{1}{2}\phi & \ldots\ldots & 3{\cdot}0795170 \\ + 637{\cdot}88 \tan^3 \tfrac{1}{2}\phi & \ldots\ldots & 2{\cdot}8047383 \\ + 163{\cdot}78 \tan^5 \tfrac{1}{2}\phi & \ldots\ldots & 2{\cdot}2142677 \\ + 19{\cdot}51 \tan^7 \tfrac{1}{2}\phi & \ldots\ldots & 1{\cdot}2903704 \\ + 3{\cdot}95 \tan^9 \tfrac{1}{2}\phi & \ldots\ldots & 0{\cdot}5967129 \\ + 0{\cdot}64 \tan^{11} \tfrac{1}{2}\phi & \ldots & -1{\cdot}8037929 \end{array} \right.$$

The comparison of this formula with the refractions in the *Connaissance des Tems,* is contained in the following Table:

The sluice gates for this lock may be formed in the usual way; but as the old plan is liable to much leakage, and is frequently disordered by obstructions; as for instance, by ice in frosty weather: the inventor proposes to obviate these imperfections by the following contrivances.

Fig. 1 is a cast-iron half-circular groove with flanges. This is let flush into the gate-post, and there secured by nails through its flanges, and into the groove is swung a cast-iron spindle by pivots in the centre of each end. To this spindle the gate is secured, as represented by the section fig. 4.

Fig. 2 is a cast-iron cock, which, being set into a brick water-course, is intended to serve the purpose of a sluice.

Fig. 3 also is a cock for the same purpose, but so formed that it may be united in the centre of a cast-iron cylindrical water-course.

By the introduction of these contrivances, it must be evident that the inconveniences arising from leakage, and obstructions, will be removed in a very material degree; and were the whole lock and its contrivances well executed, with the introduction of cast-iron water-courses, there is no reason for saying that a lock of this description may not last for a series of years. The cocks are to be turned by levers set into their respective drum-heads, which are fixed breast-high above ground; and as the cocks are situated in the centres of the several water-courses, it is presumed they are not liable to be set fast by frost.

Signed R. F. C., a candidate for the reward of 100 guineas offered by the Regent's Canal Company, for the best design of a double or single lock to produce a *saving* of water, and *facility* to the passage of vessels.

This design is accompanied by a sealed letter; and if the above plan is not approved, R. F. C. presumes that it will be returned to him on his applying to Messrs. Edwards and Lyon.

Dated September 14, 1812, and addressed to Messrs. Edwards and Lyon, Solicitors to the Regent's Canal Company, Great Russell-street, Bloomsbury.

N. B. The foregoing explanation was written on the same sheet of paper which contained the drawing of the lock, to a scale of one-eighth of an inch to a foot.

The principle and plan of this lock being now established throughout the line of the Regent's Canal, from Paddington, round the back of London, to Limehouse, in its simplest form; that is, without the several cast-iron contrivances to save leakage and avoid obstructions; induced the inventor to feel that he was entitled to the offered reward of 100 guineas; which gave rise to the following correspondence, whereby it will appear, that without

posed, if the heat decrease in the same proportion with the pressure, the depression of 1° of the centigrade thermometer will correspond to an elevation of nearly $\frac{4343}{250}$, or about $17\frac{1}{2}$ fathoms.

If we still neglect the effect of temperature, or, which is the same thing, suppose the heat to be invariably the same in every part of the atmosphere; but take into account the changes of density arising from inequality of pressure according to the law of Mariotte, we shall have another hypothesis very different from that of Cassini; the air now expanding above the earth to an unlimited extent. In this case the thermometer, at whatever elevation, would mark the same temperature; or, in mathematical language, the elevation necessary for one degree of depression would be infinitely great.

Between the two extreme cases just described we may conceive that an infinite number of intermediate ones are interposed, while the total height of the atmosphere increases from its least limit of 4343 fathoms to be infinitely great; the heat in each particular atmosphere decreasing uniformly as the elevation increases, which is the law most conformable to experience. We have thus an infinite number of different hypotheses, in all of which the refractions will be the same to the extent of 74° from the zenith; coinciding in every case with the formula of Laplace, the exactness of which is indisputably established by observation. But beyond the limit mentioned, the refractions will diverge from one another, and each particular case will have a horizontal refraction peculiar to itself. We may thus account for the inaccuracies that occur when a formula, naturally fitted to represent the refractions near the zenith, is extended, in an empirical manner, to the whole quadrant; and likewise for the shifting which such a formula requires to be made in its elements, when it is compared with exact observations made near the horizon. If indeed we consider the problem of refractions as one to be solved by observation alone, we may conclude that it is indeterminate, or admits of an indefinite number of solutions: and this is no more than an opinion, expressed in several parts of his writings, by Delambre, the astronomer of the present day, whose authority on every point of astronomical science will be allowed to be the highest.

In the hypothesis of Cassini the horizontal refraction is 1289″; and it amounts to 2394″ in the other extreme case of an atmosphere of an unlimited height. The hypothesis advanced by Thomas Simson, of a density decreasing uniformly with the elevation, is also contained in the series of atmospheres above mentioned, coinciding with the one that has its height equal to 8686 fathoms, or double that of Cassini; and in this case the hori-

 zontal

zontal refraction is 1824″. All these quantities are very different from the mean horizontal refraction determined by observation, which is 2106″ according to Laplace. But there must be one particular atmosphere in the series, which, while it possesses the general property of representing the refractions near the zenith, will likewise coincide with observation at the horizon. Now I have found that this takes place in the atmosphere that has its total height equal to 17372 fathoms, or four times that of Cassini; and the formula I have sent you was obtained by integrating the differential expression of the refraction in this hypothesis. The character of the formula may therefore be described by saying, that in all probability it will be found to coincide with observation better than any other founded on the supposition of a uniform decrease of heat.

In the atmosphere to which my formula belongs, the elevation necessary for depressing the centigrade thermometer one degree is 70 fathoms, considerably short of the observed quantity, which is about 90 fathoms. As in the series of atmospheres, there is one agreeing with observation in the quantity of the horizontal refraction, so there is also one that will agree with observation in the elevation for one degree of depression. In the atmosphere the height of which is five times that of Cassini, the elevation for one degree of depression is $87\frac{1}{2}$ fathoms, nearly equal to the observed quantity; and in this case the horizontal refraction is 2164″, or 58″ more than according to observation. In reality, neither the horizontal refraction, nor the height necessary for one degree of depression, is determined with great precision; but it is certain that on the one hand 70 fathoms is too little, and on the other 2106″ is as great a quantity as can be admitted: and hence we may infer that the supposition of a uniform decrease of heat in the atmosphere, cannot be reconciled with the astronomical refractions. But, although this be strictly true, yet the refractions are so nearly represented by the law mentioned, that the actual deviation from it must be very inconsiderable.

It would be superfluous to say any thing here of the solution of this problem contained in the *Mécanique Céleste*, the merit of which is so well known, and so justly appreciated. Both the solution now mentioned, and the one given above, seek to approach the truth by means of probable conjectures; and the ultimate results come nearer one another than was to be expected in two methods employing very different processes of investigation, and leading to formulæ of calculation that have nothing in common; the atmosphere in the one case being of indefinite extent, while in the other the total height does not exceed twenty miles.

The elevation for one degree of depression, which in my formula

mula is 70 fathoms, shows that the true physical state of the problem has not been attained. A solution agreeing with observation in this element, must proceed on a law of density that deviates a little from a uniform decrease of heat. The formulæ in the *Mécanique Céleste* likewise fall short of observation in this element: for they give a depression of 46°·24 for an elevation of 3817 fathoms instead of 40°·25 the observed quantity, being at the rate of 82 fathoms to a degree. Thus, the small differences between the French Tables and my formula correspond to a difference of no less than 12 fathoms in this element of the problem. A solution that, besides fulfilling the other conditions of the problem, should likewise agree with observation in the height necessary for depressing the thermometer one degree, would, in all probability, give the atmospherical refractions near the horizon as much above the French Tables as these exceed my formula; and in this manner, there is every reason to think, observation would be better represented. Till such a solution be found, it cannot be said that mathematical science has accomplished all that it is possible to do for the behoof of astronomy.

I am, &c.

May 8, 1821. J. Ivory.

LIV. *Some Account of a Method which may be applied to the same Purposes as Sir* Isaac Newton's *Method of Fluxions. By Mr.* Thomas Tredgold.

LETTER III.*

On the Maxima and Minima of Quantities.

To the Editor.

Sir, — In a progression of quantities, formed according to some invariable law, the quantities may be of two kinds; one of which is called constant, and the other variable.

A *constant quantity* is that which retains the same numerical value in each term of a progression of quantities.

A *variable quantity* is that of which the numerical value increases or decreases in each succeeding term of a progression of quantities.

The terms of a progression may be simple, or they may be compound. If the terms of a progression be compound, and contain both positive and negative quantities; and the positive quantities be affected with the variable quantity in a manner different from the negative ones; there will be in a progression so

* See the preceding letters at pages 177 and 200 of this volume.

constituted,

constituted, one or more terms from which the adjoining ones on each side will either increase, or they will decrease.

If there be a term from which the terms decrease on both sides, it is called a *maximum* value of the algebraical expression which forms that term.

If there be a term from which the terms increase on both sides, it is called a *minimum* value of the expression.

As an algebraic expression can be variable* only by changing the numerical value of one or more of its elements, it seems most natural and most scientific to consider the different quantities that arise from increasing or diminishing the value of the variable quantity as a progression of terms. They are sometimes compared to the ordinates of a plane curve; the only difference is, that in the one case we have a progression of geometrical magnitudes; and in the other, of algebraical quantities. When I explain my Method of Tangents (which will most probably be in my next Letter), it will appear that to determine the points in a curve where the tangents are parallel to the axis, is nearly the same thing as to find the maxima and minima of quantities.

If we consider the m^{th} term, in a progression of quantities, to be that at which a maximum or a minimum takes place; and m to be such a number that the difference between the adjoining terms shall be extremely small; then the term which precedes the m^{th} term will be nearly equal to that which follows it. Regard them as equal; that is, make $\overline{m-1}^{th}$ term $=\overline{m+1}^{th}$ term. An equation from whence the value of the variable quantity may be found, which gives a maximum or minimum value to an algebraical expression.

These terms are absolutely equal in some particular cases only, and in those cases our method is undoubtedly true. It is also true when they are not equal in consequence of a compensation of errors, in the manner I have shown to be the case in the quadrature of curves, &c. See Phil. Mag. vol. lvii. p. 201.

According to this method, it is very easy to find whether a quantity be a maximum of a minimum at the m^{th} term, by comparing it with either the term which precedes, or that which follows it. If the $\overline{m-1}^{th}$ term be less than the m^{th} term, then the m^{th} term is a maximum, if it be greater then the m^{th} term is a minimum.

As an example of the application of the method of progressions

* It is sometimes said that, while a variable magnitude passes from one state to another, it passes through all the intermediate states of magnitude; but this is true only of geometrical magnitudes, and not of numbers, or the symbols of numbers; for numbers proceed by units, that is, by steps or finite gradations.

to one of the most simple cases that occur in determining the maxima and minima of quantities, let us take the quantity $ax^r - bx^{n+r}$, in which x is variable, to find the value of x when $ax^r - bx^{n+r}$ is a maximum.

Consider x to be divided into m parts; then, in the $\overline{m-1}^{\text{th}}$ term, x will be diminished by one of these parts; and in the $\overline{m+1}^{\text{th}}$ term it will be increased by one of them. Consequently we shall have

$$\overline{m-1}^{\text{th}} \text{ term} = a\left(\frac{\overline{m-1}\,x}{m}\right)^r - b\left(\frac{\overline{m-1}\,x}{m}\right)^{n+r};$$

$$\text{and,} \quad \overline{m+1}^{\text{th}} \text{ term} = a\left(\frac{\overline{m+1}\,x}{m}\right)^r - b\left(\frac{\overline{m+1}\,x}{m}\right)^{n+r};$$

which being made equal, and reduced, give

$$x^n = \frac{a}{b} \times \frac{(1+m^{-1})^r - (1-m^{-1})^r}{(1+m^{-1})^{n+r} - (1-m^{-1})^{n+r}};$$

$$\text{or,} \quad x^n = \frac{ra}{\overline{n+r}\,b} \times \left(\frac{1 + \frac{r-1}{2} \cdot \frac{r-2}{3} \cdot m^{-1} + \&c.}{1 + \frac{n+r-1}{2} \cdot \frac{n+r-2}{3} \cdot m^{-1} + \&c.}\right).$$

But, in making the variation from the m^{th} term the same at the $\overline{m-1}^{\text{th}}$ as at the $\overline{m+1}^{\text{th}}$ term, a cause of error is introduced, which will be compensated if m be taken of such a value that omitting the latter part of the above expression will counterbalance it. Then we shall have $x^n = \frac{ra}{\overline{n+r}\,b}$, when the expression $ax^r - bx^{n+r}$ is a maximum.

This is true whether the indices be integers or fractions; negative or positive; it is essentially the same as the formula derived from the method of fluxions; and is an example of the application of the method of progressions to what is usually done by the direct method of fluxions.

I am, sir, yours, &c.

2, Grove Terrace, Lisson Grove, May 7, 1821. — THOMAS TREDGOLD.

LV. *Strictures on a Publication entitled* "CLARK'S *Gas Blowpipe.*" *By* ROBERT HARE, *M.D. Professor of Chemistry in the Medical Department of the University of Pennsylvania, &c.*

DR. CLARK has published a book on the Gas Blowpipe, in which he professes a "sincere desire to render every one his due." That it would be difficult for the conduct of any author to be more discordant with these professions, I pledge myself to prove in the following pages, to any reader whose love of justice may gain for them an attentive perusal.

In the year 1802, in a memoir republished in the 14th vol. of Tilloch's Philosophical Magazine, London, and in the 45th vol. of the *Annales de Chimie*, I had given the *rationale* of the heat produced by the combustion of the aëriform elements of water, and had devised a mode of igniting them free from the danger of explosion. I had also stated in the same memoir, that the light and heat of the flame thus produced were so intense, that "the eyes could scarcely sustain the one, nor the most refractory substances resist the other," and had likewise mentioned the fusion of the pure earths and volatilization of the perfect metals as among the results of the invention.

Subsequently in the first part of the 6th vol. of American Philosophical Transactions, an account of the fusion of strontites, and the volatilization of platinum, was published by me.

About the same time my experiments were repeated before Dr. Priestley, who gave them the credit of being quite original.

Some years afterwards, Mr. Cloud, of the United States' mint, who has distinguished himself by the discovery of palladium in gold, having purified platina so as to make its gravity equal to 22, requested me to subject it to my blowpipe. In the presence of this gentleman, I was completely successful in dissipating a portion of this pure metal. He was so much pleased with my experiments that he made an apparatus for himself, simplifying that part which was employed for holding the aëriform agents, by the omission of some appendages which were not necessary to his purpose*. Thus modified, my apparatus was introduced into use by Mr. Rubens Peale; and has for about ten years been employed by him to amuse visitors at the celebrated museum established by his father in Philadelphia.

It appears by the testimony of Professor Silliman and others, that Dr. Hope had, during his lectures, at different times within a period of eight years, employed my blowpipe and awarded the invention of it to me. A reference to the third edition of Murray's Chemistry, published before Dr. Clark professes to have

* It has been erroneously alleged that he simplified the blowpipe.

attended

attended to the subject, will demonstrate the impressions of the author of that work, as the results of my experiments which I had published are there quoted solely on my authority.

The memoir of Professor Silliman, read before the Connecticut Academy of Sciences, May 1812, and republished in Tilloch's Magazine, but which Dr. Clark has not ventured to notice, affords the most unanswerable evidence that we had anticipated him in almost every important experiment.

Mr. Reuben Haines, corresponding secretary of the Academy of Sciences, informed me in 1813, that in the laboratory of Dr. Parish in this city, a mixture of the gaseous elements of water had been inflamed while issuing in a stream from a punctured bladder previously filled with them and duly compressed. Any relaxation of the pressure was of course productive of an explosion. He on the other hand recollects, that at that time I proposed this mode of supplying the blowpipe, interposing a small receptacle (like a water valve) between the reservoir and the place of exit. Cares more imperious prevented the execution of a plan which did not promise to be better than that I had before pursued successfully.

Some time afterwards, Sir Humphry Davy's discovery of the influence of narrow metallic apertures in impeding explosions, encouraged Dr. Clark and others to hazard the use of a mixed stream of hydrogen and oxygen gas, ignited while flowing from a common recipient, instead of allowing them, as I had done, to mix only during their efflux. There is another immaterial difference in the modes of operating. In mine, hydrostatic pressure is employed to expel the gases from a vessel into which they are introduced, as generated, or by means of a bellows. In the new mode, being pumped into the recipient by one aperture, they flowed out at another in consequence of their elasticity.

Dr. Clark pretends that the process he has employed is the best. Admitting this, would it afford him any excuse for taking so little notice of mine, or attributing the discovery of it to others, especially while professing to give a *fair history* of the invention?

If I may be allowed to compare small things with great, when Mr. Cruikshank and Sir H. Davy improved the galvanic apparatus by introducing the trough, or modifying and enlarging it, did they on that account forget that Volta was the inventor of the pile? was it not still (though no longer a pile) called the Voltaic apparatus?

Dr. Clark, like many others of the same character, finding that he cannot prove himself and his associates to have the merit of originality, endeavours to deprive the real author of it, and accordingly ascribes it to Lavoisier. Had this been stated in his

first papers, his motives had been less questionable. But why does he not refer to his authorities? In other cases he is very particular in making such references.

We all know, that with a view to recompose water, Lavoisier caused the gaseous constituents of this fluid to burn within a glass globe, into which they entered by orifices *remote* from each other: but if he ever caused them to burn at a common orifice in the open air for the purpose of producing heat, wherefore is Dr. Clark the first and only person to communicate the fact to the public? How does it happen that there is no account of the invention, or of any results obtained by it, either in the elementary treatise of that great man, or in any of the contemporary scientific journals? On the contrary, in the Elements just alluded to, Lavoisier treats of the heat produced by oxygen gas, and carbon, as the highest art could produce.

Dr. Clark informs us that Dr. Thomson, now Professor of Chemistry at Glasgow, made experiments with the mixed gases seventeen years ago, but was induced to abandon the undertaking, in consequence of accidents that happened to his apparatus. Can any thing more fully display unfairness, than that abortive experiments, made subsequently to those in which I was successful, should be adduced as subversive of my pretensions?

Dr. Clark states that the Americans claim the invention on account of experiments made by me in 1802. They were published in 1802; my apparatus and my first experiments were made in 1801.

Had Lavoisier, or any other person, availed himself of the heat produced by the union of the gaseous elements of water, how could the sagacious Dr. Thomson fail in his efforts to retrace a path so well and recently trodden? or, if deriving any advantage from the experiments either of the French philosopher, or those which he so imperfectly tried, why did he conceal it when occupied during so many years in communicating to the world all his chemical knowledge in five successive editions of his system?

So far were Dr. Thomson's experiments, or his knowledge on these subjects, from reaching the facts discovered by me, that he appears to have considered the authority of one name inadequate to establish what he vainly had endeavoured to effect. Hence, until plagiarism had given them a new shape, and perhaps a false gilding, they were totally overlooked in his compilations. He neither treated of the pure earths as susceptible of fusion, nor of platinum as susceptible of volatilization, until many years after I had proved them to be so, and promulgated my observations.

Dr. Clark gives himself great credit for having first pointed out the importance of employing the gases in such relative quantities as might enable them fully to saturate each other. To me it would

would seem, where the highest heat is desired, evidently absurd to employ them in any other way; because, if either gas were present in too great a quantity to be acted upon, the excess would be worse than useless. Is it not universally an object with chemists, to use ingredients in the proportions in which they saturate each other, especially when within a given space and time the most intense reaction is to be induced? The author of this *professedly candid* publication would wish to convey the idea of my contrivance being so inferior in power to that adopted by him, that in a history of the invention he does not deem it necessary to quote my experiments, but satisfies himself with obscure allusions to them, rather in a manner to derogate than to do justice. This procedure would be unjustifiable, were the heat which he has produced decidedly greater than that produced by me. But the fact is otherwise. He fuses with difficulty oolite, Iceland crystal, and pure native magnesia. The fusion of the best magnesia of the shops, and of quick lime from pure limestone, was among my first efforts, and was mentioned in a preface, omitted in republishing my memoir. Lately I have fused a piece of oyster-shell lime, which is perhaps as pure as any to be obtained by artificial purification.

Dr. C. has employed platina in some cases to secure refractory earths while exposed to the action of his instrument, although this metal is dissipated by the heat of mine.

That in his inferences in respect to the decomposition of the earths, he did not anticipate Professor Silliman or myself, must be evident from the passages in our memoirs which I shall presently quote. I doubt, if time will show that Dr. Clark has gone much beyond *the extent of our observations* on this subject.

But while the superiority of the temperature attained by mixing the gases before emission is thus questionable, there are great and undeniable advantages in having them propelled from different reservoirs. First, A degree of security from explosion, which cannot be attained with one common recipient*. 2d, The

* Where the gases are kept unmixed in separate reservoirs, and meet only near the point of efflux in an orifice sufficiently large, as was the case with the original compound blowpipe, explosion is obviously impossible. If the orifice be made smaller, and the gases mix at a greater distance from the place of efflux, valves should be interposed in the pipes, or the gases should be kept under equable pressure, as it is possible that, if subjected to unequal pressure, the gas which is more pressed may pass from one reservoir to the other, on leaving the cocks open accidentally. This, however, is an oversight not likely to take place, as it is so evidently accompanied by a waste of the gas, that an operator will hardly be so careless as not to close the cocks when the flame is not wanted. Closing them is in fact the usual mode of extinguishing the flame.

 possibility

possibility of operating on a large scale without danger. 3d, The power of varying the relative proportions of the gases so as to oxidate, or deoxidate, as may be desirable. This power is given by the common blowpipe, though in a different way, and is well known to be very useful.

To me, it is ludicrous that the author should suppose any analogy to exist between the phænomena of the gas blowpipe and those of volcanoes.

In order to put the gas blowpipe into operation, it is indispensable that there should be hydrogen and oxygen gases, confined under moderate and equable compression, so as to flow out regularly from a common aperture, at which they may be ignited. How are these requisites to be obtained in nature? Whence the pure hydrogen or oxygen? Has Dr. Clark, or any other person, known them to be extricated in purity? Is not the former always carburetted or sulphuretted, and the latter never purer than in the atmosphere? When obtained by art, fire is requisite to liberate oxygen; but in nature, the fumes of the fire would contaminate any gas which it might evolve; and it ought not to be forgotten, that the circumstances which are favourable to the evolution of oxygen, are inimical to the liberation of hydrogen. Again, supposing the gaseous materials generated, where is the presiding demon with the genius to design, and skill to regulate, that due admixture of them which the author exults in having discovered to be necessary? And granting that there could be in nature any competent substitute for human agency in a process so intricate, by what means, in operations so rude and extensive, is that retrocession of the flame to be prevented, to obviate which, in operating with his minute apparatus, a capillary tube has been found indispensable? In subterranean caverns, the gaseous elements of water might create explosions, but could never support the permanent heat requisite to fuse an ocean of lava. The only difficulty this subject presents, is that of explaining the nature of volcanic fires, of which the incessant existence is self-evident. The access of the atmosphere is necessary to fire in all its ordinary forms. In that of volcanoes, it appears to subsist without any adequate supply of this principle. Dr. Clark, far from relieving us from this difficulty, has increased it, by alleging the necessity of another aëriform substance. A better solution, as I should suppose, was long ago afforded by a reference to the combustion of metals by sulphur, in the vapour of which some of them burn more readily than in the atmosphere. Lately, the metallic origin of earthy matter being discovered, it has been supposed possible, that at some distance from its surface the globe may consist of a great metalloidal nucleus, which acting on water, may produce intense ignition. Those who have seen the consequences of moistening

moistening quick lime, may easily conceive that tremendous effects might ensue from reaction between water and calcium, or any of the same family of substances. In this case hydrogen would be produced, but there would be no oxygen.—Of the existence, however, of subterraneous fires in volcanic regions there can be no doubt, whatever may be the theory of their origin. The obvious proximity of springs, rivers, and even of the sea itself, with the well known force of steam, renders it easy to point out the proximate cause of earthquakes, or of volcanic explosions and eruptions, without calling in the gas blowpipe to our assistance.

That Dr. Clark could not without great injustice bring forward his mode of operating, otherwise than as another method of doing what I had previously accomplished, nor his experiments, unless as an extension of those made by Professor Silliman and myself, will be perfectly evident, if it be considered that we all employed a flame of the gaseous elements of water, in the one case, mixed during the efflux, in the other, before; that the most important results in both instances will, on comparison, be found nearly the same.

The mode of confining and propelling the gases through the pipe or pipes to the place of efflux, is irrelevant to the question. There are many methods by which this object may be accomplished. The principle of the apparatus used by Dr. C. will be found the same as that of the air vault employed in England to regulate the blast of large bellows at foundries and forges. Mr. Brook was the first to apply it to the regulation of a blowpipe, and published his account of it on April 8, 1816.

I will proceed to quote and exhibit simultaneously, the observations and experiments of Dr. Clark, and of Professor Silliman and myself. As Tilloch's Philosophical Magazine is universally accessible, I shall refer to it for the memoirs of Silliman and myself: to vol. 14 for mine, to vol. 50 for his*. For Dr. Clark's experiments, commenced in 1816, I shall quote his book on the gas blowpipe, published 1819.

Experiments on Lime.

Hare, page 304. "Lime and magnesia are extremely difficult to fuse, not only because they are the most refractory substances in nature, but from the difficulty of preventing them from being blown on one side by the flame: nevertheless, in some instances, by exposure on carbon to the gaseous flame, small portions of these earths were converted into black vitreous masses. Possibly the black colour of these products of fusion, may have been caused by iron contained in the coal; for in the high temperature of the

* These experiments were performed in December 1811, and published in Bruce's Journal in 1812.

gaseous

gaseous flame a powerful attraction is exerted between iron and the earths."

Hare, page 306. "There is a peculiar species of native coal found on the banks of the Lehigh in this State, which is extremely difficult to ignite; which when exposed to a high degree of heat, and a copious blast of air, burns, yielding an intense heat without either smoke or flame, and leaving little residue. By exposure to the gaseous flame on this coal, both magnesia and lime exhibited strong symptoms of fusion. The former assumed a glazed and somewhat globular appearance, the latter became converted into a brownish semivitreous mass."

Silliman, page 109. "A piece of lime from the Carrara marble was strongly ignited in a covered platinum crucible; one angle of it was then shaped into a small cylinder, about one-fourth of an inch high, and somewhat thicker than a great pin. The cylinder remained in connexion with the piece of lime. This was held by a pair of forceps, and thus the small cylinder of lime was brought into contact with the heat without danger of being blown away, and without a possibility of contamination. There was this further advantage, (as the experiment was delicate, and the determination of the result might be difficult,) that as the cylinder was held in a perpendicular position, if the lime did really melt, the column must sink, and become at least to a degree blended with the supporting mass of lime. When the compound flame fell upon the lime, the splendour of the light was perfectly insupportable by the naked eye; and when viewed through deep coloured glasses (as indeed all these experiments ought to be) the lime was seen to become rounded at the angles, and gradually to sink, till in the course of a few seconds only a small globular protuberance remained, and the mass of supporting lime was superficially fused at the base of the column for a space of half an inch in diameter. The protuberance, as well as the contiguous portion of lime, was converted into a perfectly white and glistening enamel. A magnifying glass discovered a few minute pores, but not the slightest earthy appearance. This experiment was repeated several times, and with uniform success; may not lime therefore be added to the list of fusible bodies?"

Clark, page 47. "Lime in a state of perfect purity and in the pulverulent form being placed within a platinum crucible, and exposed to the flame of the blowpipe, its upper surface became covered with a limpid botryoidal glass, resembling hyalite; the inferior surface was quite black. Its fusion was accompanied by a lambent purple flame. This colour therefore may be considered as a characteristic hue of one at least of the oxides of calcium."

Clark, page 49, No. 6. "Compact transition limestone (limestone of Parnassus). The specimen was taken from the summit of Parnassus

Parnassus by the author. It was fused, but with great difficulty, exhibiting after fusion a white milky enamel with points of intumescence that were transparent."

Experiments on Magnesia.

Silliman, page 110. "The same circumstances that rendered the operating on lime difficult, existed in a still greater degree with respect to magnesia; its lightness and pulverulent form rendered it impossible to confine it for a moment upon the charcoal; and as it has very little cohesion, it could not be shaped by the knife as the lime had been. After being calcined at full ignition in a covered platinum crucible, it was kneaded with water till it became of the consistency of dough. It was then shaped into a rude cone as acute as might be, but still very blunt. The cone was three-fourths of an inch long, and was supported upon a coiled wire. The magnesia thus prepared was exposed to the compound flame; the escape of the water caused the vertex of the cone to fly off repeatedly in flakes, and the top of the frustum that thus remained gave nearly as powerful a reflection of light as the lime had done. From the bulk of the piece (it being now one-fourth of an inch in diameter at the part where the flame was applied) no perceptible sinking could be expected. After a few seconds, the piece being examined with a magnifying glass, no roughness or earthy particles could be perceived on the spot, but a number of glassy smooth protuberances whose surface was a perfectly white enamel. This experiment was repeated with the same success. May not magnesia then be also added to the table of fusible bodies?"

Notwithstanding the previous publicity of these results obtained by my friend and myself, Dr. Clark, in the following note, endeavours to convey an impression of the incompetency of my apparatus to fuse lime and magnesia. Note 5, page 46. "Professor Hare in America could not accomplish the fusion either of lime or magnesia by means of his hydrostatic blowpipe. See *Annales de Chimie*, tome xlv. page 126." But why overlook Silliman's experiments? It is moreover strange that an English writer should refer his readers to the French *Annales* in preference to a London magazine, for a memoir which he knew to be published in both*.

* I mentioned above that I had lately fused a piece of oyster shell lime. It was exposed to the flame within an envelope of platina foil, which was soon reduced to a fluid globule. The application of the heat being suspended (when both substances had become cold), the earth was found adhering, on the top of the metal. This enabled me to make it receive the greatest heat of the flame on renewing the process. The lime then melted into a liquid, which subsiding round the globule of platina caused it to appear after cooling as if set in enamel.

CLARK.

CLARK. *Pure Oxide of Magnesium* (Magnesia).

Fusion *per se*, extremely difficult. When the earth is made to adhere (by moisture with distilled water and subsequent desiccation) and placed upon charcoal, it is fusible into a whitish glass; but the parts in contact with the charcoal acquire an imposing pseudo-metallic lustre with a purple coloured flame.

CLARK. *Hydrate of Magnesia* (pure foliated Magnesia from America).

"This substance is incomparably refractory; with the utmost intensity of the heat of the gas blowpipe, it is ultimately reducible to a white opake enamel invested with a thin superficies of limpid glass. Its fusion is accompanied with a purple coloured flame."

Experiments on Corundum.

Silliman, page 112. "Corundum of the East Indies was *immediately* and *perfectly* fused into a grey globule." "Corundum of China the same with active ebullition."

Clark, page 56. "Common corundum (greenish grey crystallized primary corundum from the East Indies), *fusible, but with difficulty*, into a greenish coloured translucent glass nearly transparent, which at last becomes melted into a bead-like form; or otherwise exhibits upon its surface minute cavities caused by the escape of gas during its fusion. This gas is probably the same which pure silica more abundantly exhibits. A slightly coloured greenish flame accompanies the fusion of corundum."

Experiments on Sappar.

Silliman. "Sappar or kyanite perfectly and instantly fused with ebullition into a white enamel."

Clark, page 57. "This mineral, owing to its refractory nature, was used by Saussure as a supporter in experiments with the common blowpipe. It fuses very readily into a snow white frothy enamel."

Experiments on Zircon.

Silliman, page 112. "Zircon and Ceylon melted with ebullition into a white enamel."

Clark, page 58. "One of the most refractory substances; exposed to the heat of the gas blowpipe, it becomes first opaque and of a white colour; and afterwards its superficies undergoes a *partial fusion*, and exhibits a white opake enamel resembling porcelain*.

Experiments on the Spinelle Ruby.

Silliman, page 112. "Spinelle ruby fused immediately into an elliptical red globule."

* I might say here with truth, Professor Clark in England was unable to fuse zircon in his mode of operating with the gas blowpipe.

Clark,

Clark, page 58. "Fuses readily, and undergoes a partial combustion and volatilization with loss of colour and of weight. One of the solid angles of an octahedral crystal was entirely burned off and volatilized in one of these experiments."

Experiments on Silex, Alumine, Barytes.

Hare, page 304. "By exposure to the gaseous flame either on supports of silver or of carbon, barytes, alumine, and silex were completely fused. The products of the fusion of alumine and silex were substances very similar to each other and much resembling white enamel."

Silliman, page 109. "Silex: being in a fine powder it was blown away by the current of gas, but when moistened with water it becomes agglutinated by the heat, and was then perfectly fused into a colourless glass."

Clark, page 59. "Pure precipitated silica (peroxide of silicium) becomes instantly fused into an orange coloured transparent glass. The colour may be due either to the charcoal serving as a support, or, to the carbon of the oil used for making it into a paste."

On the Reduction of the Earths to the metallic State.

Hare, page 394. "The result of the fusion of barytes was a substance of an ash-coloured cast, which after long exposure sometimes exhibited brilliant yellow specks. If it be certain that barytes is an earth, these specks must have been discoloured particles of the silver support, or of the pipes from which the flame issued."

Silliman, page 113. "During the action of the compound flame upon alkaline earths, provided they were supported by charcoal; distinct globules rolled and darted out from the ignited mass, and burned sometimes vividly and with peculiarly coloured flame. From the nature of the experiments it will not be easy to prove that these globules were the basis of the earths, and yet there is the strongest reason to believe it. Circumstances could scarcely be devised more favourable to the simultaneous fusion and decomposition of these bodies: charcoal highly ignited for a support, and an atmosphere of hydrogen also in vivid and intense ignition. That the oxygen should be under these circumstances detached is not surprising; but the high degree of heat and the presence of oxygen necessarily burn up the metalloids almost as soon as produced. If means could be devised to obviate this difficulty, the blowpipe of Mr. Hare might become an important instrument of analytical research. We can scarcely fail to attribute some of the appearances during the fusion of the leucite to the decomposition of the potash it contains. This impression was much strengthened by exposing potash and soda to the compound

flame with a support of charcoal; they were evidently decomposed; numerous distinct globules rolled out from them, and burned with the peculiar vivid white light and flash which these metalloids exhibit when produced and ignited in the galvanic circuit. It is hoped these hints may produce a further investigation of this subject. This communication has already been extended further than was contemplated: but on concluding, it may be allowable to remark that there is no body, in all probability, except a few of the combustible ones, which is exempt from the law of fusion by heat."

Is there any apology for the manner in which Dr. Clark has brought himself and his friend before the public on this subject without the smallest acknowledgements for these suggestions?

Clark's *Gas Blowpipe.*

In proceeding to state the revival of two of the metals of the earths before the flame of the gas blowpipe, and of other metals under similar circumstances, it may be proper to prefix the ingenious theory of the Rev. J. Holme, of St. Peter's College, Cambridge, respecting the cause of the decomposition that takes place. "It is entirely owing to the powerful attraction which hydrogen has to oxygen at such an exalted temperature." The reduction or decomposition of oxides when exposed to the "gaseous flame*" is therefore often instantaneous, and it is as instantly followed by the combustion of the minute particles thus revived, and ultimately by the decomposition of the regenerated oxide which is a result of that combustion. Hence the coloured flame; hence also the appearance of an oxide in a state of incomparably extreme division upon the supports used whether of metal or charcoal; an irrefragable test of the revival of the metal from whose combustion this newly formed oxide has been derived.

Experiments on Strontites.

Hare, 1st part, 6th vol. American Philosophical Transactions, page 100, republished *Annales de Chimie*, vol. v. page 81. "About the same time I discovered strontites to be a fusible substance; for having obtained a portion of this earth pure, from a specimen of the carbonate of strontites of Argyleshire in Scotland, I exposed it on charcoal to the flame of the compound blowpipe after the manner described in my memoir above alluded to. It became fused into a blackish semivitreous mass in shape somewhat semi-globular."

Clark. "Here a different process is necessary; the revival of the metal is rendered more difficult, owing to the pulverulent state

* The very phrase used by me in my original memoir. See quotation on preceding page.

of

of the earth. The particles must be made to adhere before fusion can be accomplished, and this oxide being much more refractory than the preceding is almost infusible *per se*, even with the aid of the gas blowpipe." Thus he admits that a substance is almost infusible in his hands, which has been repeatedly fused under mine.

Experiments and Observations on the Fusion, Volatilization and Combustion of the perfect Metals.

Hare, page 305. "Had I sufficient confidence in my own judgement, I should declare that gold, silver and platina were thrown into a state of ebullition by exposure on carbon to the gaseous flame; for the pieces of charcoal on which they were exposed became washed or gilt with detached particles of metal in parts adjoining the spots where the exposure took place. Some of the particles of the metal thus detached exhibited symptoms of oxidation."

Combustion of pure Gold.

Clark, page 90. "As this experiment affords decisive evidence of the *combustion* of *gold*, and of course its combination with oxygen, and also exhibits the oxide under a very beautiful appearance, it may be considered as one of the most pleasing experiments with the gas blowpipe."

Experiments on Platinum particularly.

Hare, page 304. "Platina was fused by exposure on carbon to the combustion of hydrogen gas and atmospheric air. But the fusion of this metal was rapidly accomplished by the gaseous flame either when exposed to it on carbon or upon metallic supports.

"A small quantity of this metal in its native granular form being strewed in a silver spoon and passed under the gaseous flame, the tract of the flame became marked by the agglutination of the metal; and when the heat was for some time continued on a small space, a lump of fused platina became immediately formed. About two penny weights of the native grains of platina when subject to the gaseous flame on carbon, became quickly fused into an oblate spheroid as fluid as mercury. This spheroid after being cooled was exposed as before; it became fluid in less than the fourth of a minute."

Hare, 1st part, 6th vol. Philosophical Transactions, page 99, republished *Annales de Chimie*, vol. lx. page 81. "Being induced last winter to reinstate the apparatus by which these experiments were performed, I was enabled to confirm my judgement of the volatilization of platina by the observation of Drs. Woodhouse and Seybert; for in the presence of these skilful chemists I completely dissipated some small globules of this metal

of about the tenth of an inch in diameter. In fact, I found platinum to be equally susceptible of rapid volatilization, whether exposed in its native granular form, or in that of globules obtained from the orange-coloured precipitate of the nitro-muriatic solution by the muriate of ammonia."

Silliman, page 3. "Platinum was not only melted, but volatilized with strong ebullition *."

Clark, page 92. "The fusion of this metal, owing to the great improvements here mentioned in the mode of using the gas blowpipe, is now become so easy that this metal melts faster than lead in a common fire. It is no longer necessary to make use of wire in exhibiting its fusion and combustion. The cuttings which are sold by the manufacturers of platinum utensils are placed in a cupel, either mounted on a stand or held in a pair of forceps. The mouth of the jet is bent downwards so as to admit of a perpendicular direction of the gaseous flame upon the metal in the cupel. The flame is then suffered to act upon the platinum, about a quarter of an ounce of the metal being placed in the cupel at first: as soon as this begins to melt, more may be added until a cupel of the common size is nearly full of the boiling metal: and in this manner a mass of platinum weighing half an ounce at the least may be obtained in one brilliant bullet. This, when rolled out so that all air holes being removed the mass possesses a uniform density, will be found to have a specific gravity equal to 20·857. During the fusion of the metal its combustion will be often if not always apparent. It will burn with scintillation, and particles of the black protoxide of platinum, if care be used, may be caught upon a sheet of white paper while combustion is going on."

He would here evidently wish the reader to adopt the false impression, that the facility with which platinum may be fused is owing to "the great improvements" made fourteen or fifteen years after I had devised and used them. Will Britons tolerate such conduct in their professors?

Silliman, last page. "The experiments which have now been related in connexion with the original ones of Mr. Hare, sufficiently show that science is not a little indebted to that gentleman for his ingenious and beautiful invention. It was certainly a happy thought, and the result of very philosophical views of combustion, to suppose that a highly combustible gaseous fluid, by intimate mixture with oxygen gas, must when kindled produce in-

* The fusion and combustion and complete dissipation of platinum, gold, silver, nickel, cobalt, and most of the metals, and the fusion of the principal earths and of their most refractory compounds, by the use of Professor Hare's compound blowpipe, have been *the familiar and easy class experiments* of every course of chemistry in Yale College for these eight years.—[Ed.]

tense

tense heat, and it is no doubt to this capability of perfectly intimate mixture between these two bodies, and to their great capacity for heat, that the effects of the compound blowpipe are in a great measure to be ascribed."

Clark, Journal Royal Institution, page 122. "I consider this improvement of the blowpipe, one of the most valuable discoveries for the sciences of chemistry and mineralogy that have yet been made." And thus does he modestly claim to his modification the whole merit of the discovery; for, it must be observed, he does not, in saying "improvement of the blowpipe," allude to the compound blowpipe contrived by me, but to the ordinary blowpipe of the mechanic or mineralogist. Other instances might be adduced; but it is presumed that more than enough has been brought forward to show, that if the merit of this invention is to be awarded according to the motto '*suum cuique*,' adopted by Dr. Clark, there would be little left for himself and his coadjutors.

☞ To the foregoing Dr. Hare subjoins some drawings of his compound blowpipe in its different forms, and of some varieties of apparatus which may be used for supplying it with hydrogen and oxygen gas, but which may be readily conceived by those possessing the volumes of the Phil. Mag. to which he has referred, without our enlarging the present article. We believe it is allowed by most men acquainted with this subject, that Dr. Clark has not acted towards Dr. Hare with any over share of honest liberality.

LVI. *Some Account of the Dugong. By Sir* THOMAS STAMFORD RAFFLES, *Governor of Sumatra; communicated in a Letter to Sir* EVERARD HOME, *Bart. V.P.R.S.**

MY DEAR SIR,—I HAVE now the pleasure of communicating to you the desired information concerning the dugong. At Singapore, in June last, I had the good fortune to meet with one of these animals, and Messrs. Diard and Duvancel, two French naturalists, employed under my authority, undertook the dissection of it; and have sent a dissertation upon it to Sir Joseph Banks. This does not interfere with my sending to you, as I promised, an account of it. I was present at the dissection; and the following observations, as far as they go, may be depended upon. I have read them over to Dr. Wallick and General Hardwicke, and they concur in opinion as to the correctness of the description. I have the pleasure to acquaint you, that General Hardwicke has just now got a small dugong, four feet six inches long,

* From the Transactions of the Royal Society for 1820. Part II.

which

which I have succeeded in persuading him to send home to you for dissection, and you will receive it by the next ships.

The dugong which we examined measured eight feet and a half in length, and afforded no less interest under the knife than satisfaction on the table, as the flesh proved to be most excellent beef. Our entertainment was truly marine; for we had on the same day discovered those Neptunian sponges which General Hardwicke has since described, and which served us as goblets.

In form the dugong resembles the common cetacea, having, like them, a broad horizontal tail, and two pectoral fins without nails. The skin is smooth, thick, blueish above and whitish beneath, with a few remote and scattered hairs. The mammæ (in the two male individuals examined) are small, and situated on the breast, immediately below the pectoral fins.

Head small in proportion, obtuse, and of a peculiar form. Upper lip very large, thick, and obliquely truncated, forming a short, thick, and nearly vertical kind of snout. The surface of the truncated portion is covered with soft papillæ, and is also furnished with a few bristles. Two short tusks project straight forward from the extremity of the upper jaw, and are nearly covered by the upper lip, which is very moveable, and tumid at the margin. The lower lip is much smaller, and resembles a round or oblong chin The margin of both lips is furnished with strong coarse bristles. There are no incisors in either jaw (the tusks above mentioned being more properly defences), their place being supplied by the rough bristly surfaces of the palate and jaws, which serve as rasps, to enable the animal to browse upon the *algæ* and other submarine vegetables. To facilitate this still further, the anterior part of the jaw is bent downwards at an angle, in such a manner as to bring the mouth into nearly a vertical direction. There are no canine teeth. The molares are twelve in number, six in each jaw, placed far back on the horizontal part. They are cylindrical, with flat crowns; the first are somewhat oblique, and worn to a kind of point; the second are perfectly flat; but the last are composed of two parallel and adhering cylinders. They are short, and scarcely project from the gums. The tongue is small and short. The nostrils are situated on the summit of the upper jaw, where it makes its curvature downwards. They penetrate obliquely, in such a manner that the upper semilunar edge, pressing upon the lower surface, forms a perfect valve. The eyes are small, and situated on the sides of the cranium. The aperture of the ears is so small as with difficulty to be perceived, and is situated at some distance behind the eyes.

Body

Body rounded, diminishing to the tail, and without any vestige of dorsal or ventral fins. The place of the anterior extremities is supplied by fins, which offer no appearance of nails, but are somewhat verrucose on their anterior margin. They are thick and fleshy, and neither from their form nor size capable of supporting or assisting the animal out of the water.

Tail broad, horizontal, and of a crescent or semilunar form.

Dissection.

Skin three quarters of an inch thick, with little adipose matter, and yielding no oil.

The cavity of the abdomen large.

The stomach is large; and the relative position of the cardiac and pyloric orifices is nearly as in the human subject. It has two appendages, which open into it near the junction of the duodenum. Membrane of the stomach thick, internal surface smooth, and not corrugated into plicæ. The stomach and its appendages were distended with *fucus* or sea-weed, but little masticated or altered. Intestinal canal long. Small intestines uniform. Cæcum very large, somewhat curved, and containing a portion of partially digested sea-weed. Colon exceeding the small intestines in diameter by one third, very uniform, and with few or no contractions. Liver of moderate size, consisting of two large and distinct lobes, connected by a smaller one somewhat tongue-shaped, and a fourth which was very small, on the posterior side. Gall bladder little distended, and situated beneath the third and tongue-shaped lobe. Spleen very small, not exceeding three inches long and one inch thick, attached to the left side of the stomach. Pancreas lying below the duodenum. Kidneys in their usual place, and large. Bladder much contracted, not exceeding the size of an egg, but from the thickness of its coats is probably capable of much greater distension.

Testicles situated a little below the kidneys, egg-shaped, flattened, partly embraced by a very perceptible epididymis.

Penis large; while collapsed entirely concealed within the prepuce. The glans consists of two lobes, separated or cloven above, in such a manner as to give the whole the appearance of the cloven foot of a ruminating animal. The urethra opens on a small tubercle or papilla between the lobes of the glans.

In the thorax, the thymus gland is particularly large, black and friable under the fingers, and occupying the space between the folds of the mediastinum.

Lungs two, distinct, of an elongated form, not lobulated, and situated posteriorly, one on each side; their substance of the usual

usual mottled colour. The trachea bifurcates very high up, and the two branches diverge to their respective lungs.

Heart situated on the left side, double; that is to say, having the ventricles entirely separate at their points, and only connected at the upper part, or base. Each side possesses a ventricle and auricle, with the usual valves, and without any communication between the right and left sides. The left ventricle, which gives off the aorta, is stronger and more muscular than the right, whose cavity is larger, and coats thinner.

Of the skeleton, a few observations will suffice.—The skull is remarkable by the peculiar manner in which the anterior part of the upper jaw is bent downwards, almost at a right angle, so as to form a kind of beak. The lower jaw is truncated in such a manner as to correspond, and become parallel with the elongated portion of the upper jaw. This portion of the lower jaw has eight alveolar excavations, which are sometimes empty, and sometimes contain the rudiments of teeth.

The vertebræ are fifty-two in number, seven to the neck, eighteen to the back, and twenty-seven to the tail.

Ribs, eighteen on each side.

Sternum nearly a foot long, bifurcate at the apex, and articulated to the cartilages of the upper ribs.

There is no pelvis or posterior extremities, but there are found opposite to the eighth or tenth lumbar vertebra two bones, one on each side, lodged in the flesh, which are narrow and flattened, and not above five or six inches in length. Scapulæ broad and thick; humerus short and strong, as is also the radius and ulna. The whole of these are firmly articulated to each other; and though externally the fins offer no appearance of fingers, all the corresponding bones are found complete even to the last phalanges.

The food of the dugong appears to consist exclusively of *fuci* and submarine *algæ*, which it finds at the bottom of shallow inlets of the sea. The position and structure of the mouth enables the animal to browse upon these vegetables, much in the same manner as a cow in a meadow; and the whole structure of the masticating and digestive organs shows it to be truly herbivorous. The flesh resembles young beef, and is very delicate and juicy. The individual, of which the skeleton and intestines are now sent to England, was taken at Singapore in June 1819.

According to the information given by the natives, the dugong is never found on land, or in fresh water, but generally in the shallows and inlets of the sea, where the water is only two or three fathoms deep. During our short possession of Singapore, (not more than six months) four of these animals have been taken; but the greatest number is said to be caught during the opposite

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or northerly monsoon, when the sea is calmest, near the mouth of the Johore river, in the inlet of the sea between Singapore Island and the main. They are usually taken by spearing (at which the natives are particularly dexterous) during the night, when the animals give warning of their approach by the snuffling noise they make at the surface of the water. The first object is to secure and elevate the tail, when the animal becomes perfectly powerless, and at their disposal. They are seldom caught above eight or nine feet in length; but how much larger they grow is not ascertained, as, when they exceed this size, their superior strength enables them to make their escape when attacked.

The Ikan dugong is considered by the Malays as a royal fish, and the king is entitled to all that are taken. The flesh is highly prized, and considered by them far superior to that of the buffalo or cow. They distinguish two varieties, the duyong *bumban*, and the duyong *buntal;* the latter much thicker and shorter in proportion. The breasts of the adult females are said to be large. The affection of the mother for its young is strongly marked; and the Malays make frequent allusion to this animal, as an example of maternal affection. When they succeed in taking a young one, they feel themselves certain of the mother, who follows it to the margin of the sea, and allows herself to be speared or taken with the greatest ease. The young have a short sharp cry, which they frequently repeat; and it is said they shed tears. These tears are carefully preserved by the common people as a charm, the possession of which is supposed to secure the affections of those to whom they are attached, in the same manner as they attract the mother to her young. This idea is at least as poetic, and certainly more natural than the fable of the Syren's song.

I remain, my dear Sir, yours truly,

THOMAS STAMFORD RAFFLES.

Dimensions.	Ft.	In.
Total length of the animal	8	6
Greatest circumference	6	0
Length of the head from the nostrils to the occiput	1	3
——— from the nostrils to the end of the snout	0	3½
Width of the snout	0	9½
Depth of do.	0	4½
Length of the chin	0	5
Breadth of do.	0	5½
Distance from the nostrils to the eyes	0	6½
——— the eyes to the ears	0	6½
——— the eyes to the fin	1	5½
Length of the fins	[illegible]	4
Breadth of do.	0	8

	Ft.	In.
Breadth across the belly from fin to fin	1	11
Distance between the mammæ	1	5
Breadth of tail from tip to tip	2	7
Circumference of the root of the tail	1	9
Distance from the anus to the centre of the tail ..	2	9
——— from the anus to the penis	1	2
Total length of the intestines	115	0
—— do. of small intestines including the cæcum ..	44	0
—— do. of great intestines	72	0

With this account, Sir T. S. Raffles sent me a copy of some observations in French, by Messrs. Diard and Duvaucel, upon the stomach of the dugong. Sir T. S. Raffles mentioned that these observations formed part of a memoir written by those gentlemen. Under these circumstances, I have not felt myself authorized to lay them before the Society, along with those made by Sir Thomas Stamford Raffles, which I consider of too much importance to be delayed.

EVERARD HOME.

LVII. *A Series of Queries addressed to Dr.* BURNEY *of Gosport, regarding* SHOOTING STARS *and* METEORS, *with some Suggestions on the same Subject to the* ASTRONOMICAL SOCIETY *of London, for making these Phænomena available in settling the Longitudes of Places, and towards extending our Knowledge of the very numerous planetary and satellitic Bodies, composing the Solar System. By Mr.* JOHN FAREY *Sen.*

To the Editor.

SIR, — AMONGST the many learned and ingenious Individuals in this Country, who periodically record their Meteorological Observations in our Scientific Journals, I have observed none others who do so, on a scale so complete and comprehensive, as *Dr. William Burney*, Master of the Naval Academy at Gosport *; in the "Annals of Philosophy." Under the head of "Atmo-

* Dr. Burney states, that the bason of his Barometer, is fixed about 30 feet above high-water mark: he would greatly oblige me and others, if he would ascertain its exact height above *low-water* mark, and communicate the same for insertion in your Magazine, with a series of observations at 8, 9, 10, 11, and 12 h. on each *second Monday* of the Month, for comparison with the simultaneously recorded observations of others. If any gentlemen resident on the Southern coasts of the Isles of Wight, Purbeck or Portland, could be prevailed on to make and send corresponding Observations, the value of each one of such observations, and of the many others now making in the interior, would thereby be greatly enhanced.

spheric

spheric Phænomena," the Doctor has of late years recorded, the number of his observations on small *Meteors* or *Shooting Stars;* these, in the year 1820 were, in January 7, February 2, March 1, April 2, May 2, June 1, July 15, August 80, September 10, October 4, November 2, and December 5; making 131 in this year: In 1819 the annual number of such observations was 121.

The singular fact, of the month of August having furnished so very disproportioned a number of these observations, is accompanied by the mention, that 35 of these were observed in one hour, which preceded midnight on the 9th of August last—they shot in different directions, and three of them, whose visible paths lay between the constellations *Lyra* and *Ursa Major*, were caudated or appeared with tails; and the Doctor adds, "their sparkling trains having been left brilliantly illuminated, for several seconds of time subsequent to the disappearance of the ignited bodies: this indeed was the grandest display of meteors we ever remember to have seen in so short a period, arising from the very gaseous or inflammable state of the air."

I have been induced to trouble you on this occasion, principally on account of the sentence last quoted, with the hope that this may meet the eye of Dr. Burney, and induce so indefatigable an observer and able a calculator, to commence a series of more minute observations on Shooting Stars and Meteors, with a view to the scientific solution of the following Queries, viz.

1st. Whether a small degree of planetary Light, like that of the Moon when only one or two days old, be not sufficient to obscure numerous of the smallest and medium shooting Stars? *; and the Full Moon able to obscure the whole of them, and also render invisible the smaller Shooting Meteors?

2nd. With a clear Sky and the absence of planetary Light, are not Shooting Stars *of very frequent occurrence*, at all seasons? and in every constellation, or portion of visible space?

3d. Whether some of these do not shoot in all directions?; although more frequently they may incline downwards or towards the horizon, than upwards?

4th. If of two Observers of the same shooting Star or Meteor, one should instantly cause his eyes pretty accurately *to follow the moving Light*, and the other should not so follow it; would not these observers differ in their conceptions of what they had seem?; so that one might describe a tail or train of light to be left for a short time, and the other mention no such appendage or occurrence.

5th. After comparing together a long series of Observations, made

* I mean such as Dr. Burney must often have observed, amongst the 131 shooting stars mentioned in the Text.

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with all the precautions which an experimental solution of the above four questions may suggest to an intelligent and unprejudiced Observer, and attending to *the gradations* in every particular of the appearances; such as, from faint to brilliant or dazzling, in the degree of light emitted; from very small to large, in apparent magnitude; from short to very long, in the apparent course; from slow to very swift, in the apparent motion, &c.; will it not appear extremely probable, if not certain, that the faintest shooting Star, appearing but for the fraction of a second of time through an arc of a few degrees only, and the most brilliant and obtrusive Meteor, holding on its course through many seconds, across all the visible horizon, and perhaps at intervals exploding, superficially, and throwing down its stony fragments to the Earth,—will it not, it is asked, appear, that we have such a connected chain of facts, as to force the conclusion, that the whole of these appearances are referable *to one class of Bodies?*

6th. If with these lights thrown on the subject, two or more Observers at different places, not too far asunder, whose respective bearings and distances are known, make *corresponding* or *simultaneous observations*, of the instant of appearance (by a well regulated clock), the direction of motion (nearly), and the Track amongst the fixed Stars (with other particulars), of all the shooting Stars or Meteors which may appear in or near to some Constellation, and through a given number of Hours, both of which last, the Constellation and the Hours, had previously been concerted between the Observers; will not the necessary *data* thus be obtained, for certainly *identifying* any such luminous Bodies, as may have been simultaneously observed?; and also for very nearly calculating their heights, and over what Places on the earth's surface they were moving, when so observed?:—and by a comparison of the apparent directions of moving at the different Places, will not approximations to the direction (in azimuth) of the motions of such Bodies be obtained?

7th. When a considerable number of new observations and calculations shall thus have been obtained, and collated with those results which have already been published, regarding several Meteors, and with regard to the heights of a few shooting Stars: if it shall appear, that the faintest and shortest courses of the shooting Stars while visible, were severally performed within, but not far within the nearly spherical shell of air which surrounds the Earth, as an Atmosphere?; also, that the longer and more bright courses of the shooting Stars observed, were generally performed somewhat deeper within the Atmosphere?; also that the smaller class of Meteors, having a longer and more

more brilliant course, were generally moving at the time, in a lower stratum of air than the shooting Stars?; and lastly, that the greatest and most striking of our Meteors, which have yet been subjected to satisfactory calculation, were then moving, the lowest of any in the atmosphere?; will not such a chain of facts as these, be sufficient for referring all these Bodies, to the class of *Satellitulæ* of the Earth?

8th. If it shall appear (as already hinted) that a large proportion of the shooting Stars and Meteors have a *downward* course; which appearances, may in many instances be, merely the effects of *the perspective* of courses, which would no where meet or come in contact with the Earth?; also if many of them *vanish instantaneously in clear Sky* (and not behind a cloud, as too often has been said and written) while so descending,—will not these and other circumstances, when attentively and philosophically weighed, lead to the inference, that these *Satellitulæ* are principally, if not exclusively visible, in the latter portions of their perigeal courses, across the atmosphere?;—and to the further inference (for establishing which, many other facts might be adduced) that the vast *friction of the Air* (even in the highest situations in which we see shooting Stars move) in the first portion of such perigeal course through the Atmosphere, has occasioned the candescence, and the brilliant temporary combustion which we witness, in the latter portion of such course?:—and further, that the passing of a Satellitula out of the atmosphere, occasions the sudden extinction of its light; which may so commonly be observed, and perhaps can no otherwise be accounted for?

9th. Would not a constantly recurring retardation (at intervals not greatly different from 9 hours, perhaps) of the projectile motion of a *Satellitula*, appearing occasionally at some past era of the world, as a faint shooting Star to its Inhabitants, have occasioned such Satellitula to move in a sort of Elliptical Spiral, around the Earth's centre?;—and on principles deducible as above, would not such a *Satellitula*, at first very slowly, and afterwards (as it had a longer and larger course, through a denser and denser medium of Air, while in perigeo) more rapidly increase in the brightness and length of its visible or shooting course?;—has not, with respect to many at least of these Satellitulæ, the transition taken place, from a shooting Star to a Meteor?;—may not some of these latter, after appearing as a large and very dazzling Meteor (perhaps of a far more imposing aspect than any which we have upon authentic record) have, by long-repeated explosive exfoliations (producing Meteoric showers of stones) become so reduced in size and so roughened in shape, as, through the Air's resistance, no longer

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to be able to over-top some mountain chain on the Earth's surface, with which it has come in forcible contact?; and may we not with probability thus account, for some of the wonderful stories, of huge burning Rocks (supposed by some to be volcanic) being hurled, and mountains being split, and in part overturned thereby, &c. which in vague histories, have been mentioned?

I shall not now further extend these Queries, by the mention of several other points, which engaged my very anxious attention about 20 years ago, while making a series of simultaneous observations on Shooting Stars and Meteors, with your able Correspondent Mr. Benjamin Bevan, at Woburn, and at Leighton in Bedfordshire: because I rather fear, that the present generation of observers and calculators, will (like myself) shrink from the appalling difficulties of the task, of attempting to ascertain *the periodic times*, the successive *places of perigeal appearance*, &c. of any considerable number of those terrestrially revolving Bodies, which I have proposed to name *Satellitulæ:* which task, nevertheless, I believe it to be possible, to have accomplished, and that *the return*, at certain Places, of certain *Satellitulæ* might be foretold, with incomparably more certainty and exactness, than the return of any one of the numerous Comets or excentric Planets of our System, can yet be foretold.

In the mean time, the sixth of my Queries above, will I believe, be found to suggest, *an important use of these shooting Stars and Meteors*, whether they really be *Satellitulæ* with Orbits capable of determination, or not, viz. *as luminous Signals, which can be correctly and simultaneously observed, over a great part of England and Wales, as the means*, by frequent repetitions, *of accurately settling the Longitudes of Places on Land:* or on the other hand, of occasionally furnishing *the true Time*, to those Persons knowing their Longitude, and being possessed of a good Clock, who may not be furnished with a Transit or other Instrument, requisite for obtaining their Time.

Towards these desirable ends, I beg the liberty of earnestly recommending to the Council of the *Astronomical Society* of London, to take this subject into their serious consideration*, and if it should be thought necessary, requesting the aid of the Board of Longitude for pecuniary assistance, towards making and re-

* I was glad to observe in the inaugural Speech of Sir Humphry Davy, from the Chair of the Royal Society, reported in p. 151, of your last volume, the attention of the learned and ingenious pointed to this subject:—on which for 20 years past, I have been endeavouring to arouse them; but hitherto with few visible effects, beyond occasioning some Electrician, some Volcanist, or other Individual, in reality unacquainted with the subject, to dogmatize thereon.

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cording observations, under the direction of its learned Members, of the exact *time* of appearance, for the meridian of Greenwich, and every other observable circumstance, regarding such *shooting Stars and Meteors*, as may appear moving, across one or more aërial fields of observation, to be chosen vertically to one or more known Places in England, within such parts of every clear night when the Moon does not powerfully shine, as they may judge proper, and previously announce to the public: in order that ingenious Persons in every part of the Kingdom, may be enabled to make simultaneous and corresponding observations, directed either towards the determination of Longitudes, or towards ascertaining the Orbits and the times and places of appearance, of some of the very numerous *Satellitulæ*, which are believed to be continually making their rapid revolutions around our Planet, by

Sir, your obedient servant,

30, Howland-street, Fitzroy-square, May 8, 1821. — JOHN FAREY Sen.

LVIII. *On the Cure of Scrofula by means of Vital Air, and the Use of the Juice of Sorrel. By* ROBERT JOHN THORNTON, *M.D. Member of the Royal London College of Physicians, and Lecturer on Botany at* GUY'S *Hospital.*

To the Editor.

SIR, — THE following cases are of so extraordinary a nature, that they merit a place in your most valuable Philosophical Magazine, now become a national work, equally honourable to yourself, as to the philosophic world, which has so long, and to some individuals unexpectedly, supported a pure work of intellect.

First Case.—Miss Burstall, sister to Mr. Burstall, Charlotte-street, Rathbone-place, æt. 20, had a tumour extending round the neck, of a frightful size. It had been increasing for above four years, and began to press upon the windpipe, and impede her breathing, resisting the applications, internal and external, of the most eminent surgeons; she waited upon Mr. Thomas, a most skilful operator, with the bold resolution of having it removed with the knife. He assured her no operation could be performed; and as for a cure, that was impossible. Without hope, this young lady applied to me; and without leaving the smallest scar, I dissolved, as in the case of Miss Homer, before published in the Phil. Mag., this terrific tumour, which separated into seven or eight glands, and finally disappeared, when she became a most lovely young lady, with a fine florid complexion. She has enjoyed now uninterrupted health for three years.

Second Case.—Miss Cunningham, daughter of a gentleman in the house of Farquhar, Broad-street, æt. 15, had a similar tumour

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in the neck, which being deemed scrofulous, she went to the sea-side, and bathed in the sea. Mrs. Clementson, a lady residing in Oxford-street, near the Pantheon, who had been two years under Dr. Bree for a liver complaint, and when given over by all her friends and relations, was restored to perfect health by the inhalation of vital air; being at the sea-side for pleasure, seeing this young lady, she desired her to persist in continuing at the sea, as medically advised; yet fearing she would return home no better, advised her afterwards to consult me. The father was incredulous; but he consented to her trying the vital air, aided by other remedies; and he soon became a convert, for the benefit was immediate, and the cure was soon accomplished, and she has remained perfectly well above a twelvemonth.

Third Case.—Miss Ridley, æt. 12, daughter of a shoemaker in St. Paul's Church-yard, had a similar tumour of the neck, which was so large as to affect her speech. The father, knowing of these cases, applied to me for advice for his daughter, when I recommended to her the vital air; and at the same time strengthening up the system, she was likewise soon restored to perfect health, and has continued well now above a year.

Fourth Case.—Miss Mary Dixon, daughter of a copper-plate printer, Tottenham-street, æt. 18, had a similar enlargement of the glands, and she underwent the same process: the tumour was dissolved, the vital air took off the pallidness of her complexion, and she enjoys now most excellent health.

Observations.—1. I could add considerably to this list of cures, accomplished by the aid of the vital air, did I not think it unnecessary; for the number here given shows, that the same result might be expected in similar cases.

2. The usual applications of stimulants to the neck, and internal strengthening remedies, as the burnt sponge and bark, were had recourse to, which before were ineffectual without the vital air.

3. The quantity of vital air inhaled was from four to six quarts, diluted with three times that quantity of common air.

4. By measurement the diminution was progressive, and in one case decreased five inches.

5. All the glands of the neck seemed to participate in the same disease.

6. It may be asked, Is scrofula hereditary? My answer is, that I have known children to be scrofulous where no taint whatever could be traced to the parents, and one child to be found scrofulous, when all the others had no such disposition. These are finely strung, as the blood-horse, and therefore more subject to disease; but this is a peculiar temperament; the disposition, therefore, may be hereditary, but not the disease.

7. How

7. How is the disease created? In all these cases I found the patients were in the habit of drinking cold water, and that at night; and nothing is more injurious than cold applied when the body is warm, or hot.

8. Have we any popular remedy for scrofula? Miss Smith, daughter of Mr. Smith, at the Feathers, Brown-street, Edgeware Road, had the glands of the neck affected, with other symptoms of scrofula: she had been for two years under different surgeons, without benefit, when she applied to me. I ordered the juice of sorrel to be taken three or four times a day, and the same to be applied on linseed meal to the wounds, which when sufficiently drawn were to be healed by Turner's Cerate, or any simple ointment.

I have now before me a list of upwards of two hundred cures performed with the juice of sorrel (*Acetosa acetosella*) taken internally, and applied outwardly. Mrs. Shoubert, of Hackney, is now before me, who was given over by Mr. Toulmin, of Hackney; but by inhaling the vital air her health was restored in a most extraordinary manner. A scrofulous wound near the clavicle appeared; but by means of sorrel juice this was removed. You may recollect, that this remedy we derived from the popular use of it in Ireland; and I have myself a little son, too young to inhale the vital air, who, after being cured of water in the brain, which left him paralytic on one side, from debility became scrofulous; and having battled through the winter, with no healing of the wounds, I have stationed him on Highgate Hill, opposite the Pound, where there is in front of the house a field filled with the wild sorrel, which he greedily eats, and he also drinks the juice; and it is applied to his wounds with the most manifest advantage: such is my confidence, from an experience of thirty years and upwards, in this simple remedy, so well adapted for very young children. When the wounds are sufficiently drawn with the sorrel juice mixed with the linseed meal, I afterwards apply any common ointment, still continuing it internally.

Hoping this communication may have the desired effect of extending the knowledge and use of the most mild, yet efficacious remedies, in the most dire of our diseases,

I have the honour to conclude,

Dear sir, &c.

ROBERT JOHN THORNTON.

LIX. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. GROOBY.

[Continued from p. 271.]

1821.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
July	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	3·94	7·82	57·24	40·70	29·96	57·09	0·16	29·96	15·82	11·03	56·58	22·21	48·39	51·29	57·34	24·22	48·78	32·69
2	97	85	27	72	98	11	18	97	84	04	58	22	39	29	34	21	77	68
3	4·00	88	30	75	30·01	13	20	99	85	05	59	23	39	29	33	20	76	67
4	04	91	33	77	04	15	22	01	86	06	60	23	38	29	32	19	75	66
5	07	95	36	79	06	17	25	03	87	07	61	24	38	28	31	19	74	65
6	11	98	39	82	09	19	27	05	88	08	62	25	38	28	30	18	73	64
7	14	8·01	42	84	12	21	29	06	90	09	62	26	38	28	30	17	73	63
8	17	04	45	87	15	23	31	08	91	10	63	27	38	27	29	17	72	62
9	20	08	48	89	18	27	34	10	92	11	64	28	38	27	28	16	71	61
10	23	11	51	92	21	28	36	12	93	13	65	29	38	27	27	15	70	60
11	26	15	54	95	24	30	39	14	95	15	67	31	38	27	26	15	69	59
12	29	18	57	98	28	33	42	16	96	16	68	32	38	27	26	14	69	58
13	32	21	60	41·00	31	35	45	18	98	18	69	33	38	26	25	13	68	57
14	35	25	63	03	34	37	47	21	99	20	71	35	38	26	24	13	67	56
15	38	28	66	06	38	39	50	23	16·01	21	72	36	38	26	23	12	66	55
16	41	31	69	09	41	42	53	25	02	23	73	37	39	25	22	11	65	53
17	44	35	72	12	44	44	55	27	04	24	74	39	39	25	22	10	64	52
18	47	38	75	14	47	46	58	29	05	26	76	40	39	25	21	10	63	51
19	50	42	78	17	51	48	61	31	07	28	77	41	39	25	20	09	62	50
20	53	45	81	20	54	51	64	33	09	29	78	43	39	25	19	08	61	49
21	56	48	85	23	58	54	67	36	10	31	80	45	40	25	19	08	60	47
22	59	52	88	26	61	56	70	38	12	33	81	46	40	25	18	07	59	46
23	62	55	91	29	65	58	73	41	14	35	82	48	40	25	17	07	58	45
24	66	58	94	32	69	61	76	43	16	37	84	50	41	25	16	06	57	44
25	68	61	97	35	72	63	78	45	18	38	85	51	41	26	16	05	56	42
26	71	65	58·01	38	76	66	81	48	19	40	87	53	42	26	15	05	55	41
27	74	68	04	41	80	68	84	50	21	42	88	55	42	26	14	04	54	40
28	76	71	07	44	84	71	87	53	23	44	89	56	43	27	14	04	53	38
29	79	75	11	47	87	73	90	55	25	46	91	58	43	27	13	03	52	37
30	82	78	14	50	91	76	93	58	27	48	93	60	44	27	12	03	51	36
31	85	81	17	54	95	79	96	60	29	50	95	62	45	28	12	02	50	34

1821.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
July	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	51·05	2·49	9·72	30·50	29·46	32·64	41·05	55·87	48·23	6·20	34·50	46·71	10·59	22·69	38·20	47·49	53·58	11·35
2	05	49	71	50	46	64	06	87	24	21	52	72	60	71	23	52	61	38
3	04	48	71	49	46	64	06	88	25	23	53	74	62	73	25	55	64	32
4	[illegible]	47	70	49	45	64	06	88	27	24	55	76	64	75	27	58	67	35
5	[illegible]	46	69	48	45	64	06	89	28	25	56	78	66	77	30	62	70	49
6	01	45	69	48	45	64	06	89	30	27	58	80	68	79	32	65	73	52
7	01	45	68	47	45	64	06	90	31	29	60	81	69	81	35	68	76	56
8	00	44	67	47	44	64	07	90	32	30	61	83	71	83	37	72	79	59
9	50·99	43	67	46	44	64	07	91	34	32	63	85	73	85	40	75	82	63
10	98	42	66	46	44	64	07	91	35	33	64	86	74	86	42	78	85	66
11	97	41	65	45	43	64	07	91	36	34	65	88	76	88	45	82	87	69
12	96	40	64	44	43	64	07	91	37	36	66	89	77	89	47	85	90	72
13	95	39	62	43	43	63	06	91	38	37	68	91	79	91	49	88	93	76
14	94	38	61	43	42	63	06	91	39	38	69	92	80	92	52	92	95	79
15	94	38	60	42	42	63	06	91	40	39	70	94	82	93	54	95	98	82
16	93	37	59	41	41	63	06	91	41	40	71	95	83	95	56	98	54·01	85
17	92	36	58	40	41	62	06	91	42	41	72	97	85	96	59	48·01	03	88
18	91	35	57	39	40	62	06	91	43	43	74	98	86	97	61	05	06	92
19	90	34	55	39	40	62	06	91	44	44	75	47·00	88	99	64	07	08	95
20	89	33	54	38	39	61	05	90	45	45	76	01	89	23·00	66	11	11	98
21	88	32	52	37	38	60	05	90	46	45	77	02	90	01	67	13	13	12·01
22	87	31	51	36	38	60	05	89	46	46	77	03	91	02	69	15	15	04
23	86	30	49	35	37	59	04	89	47	47	78	04	92	02	71	17	18	07
24	85	29	48	34	36	58	04	88	48	47	79	05	93	03	73	19	20	09
25	84	28	47	33	35	57	03	87	48	48	80	06	94	04	75	21	22	12
26	83	27	45	32	34	57	03	87	49	49	81	07	95	05	77	23	25	15
27	82	26	44	31	34	56	03	86	49	50	81	08	96	06	78	25	27	18
28	81	25	42	30	33	55	02	86	50	50	82	09	97	07	80	27	29	21
29	80	24	41	29	32	55	02	85	50	51	83	10	98	08	82	29	31	24
30	79	23	39	28	31	54	01	84	50	51	83	12	11·00	08	84	31	34	27
31	78	22	38	27	30	53	00	83	51	51	84	13	01	09	85	33	36	30

LX. *On the American Sea Serpent.*

The voyage undertaken by Capt. Rich of Boston in 1818, for the purpose of taking the sea-serpent of which so much had been reported in 1817, but which voyage terminated in his catching a thunny, or horse mackerel, served for a time to throw discredit on all the statements which had been published respecting this wonder of the deep. The subject, however, has undergone fresh discussion, and Professor Bigelow of Boston has collected and published so many documents (in Silliman's Journal), as seem to put the truth of the existence of this serpent beyond all doubt.

It is a curious enough fact, that an account of a similar fish had been sent to the American Academy of Sciences sixteen years ago, but had been mislaid. This document is now brought to light. It is a letter dated Wiscasset, May 22, 1804, addressed to the Hon. John Q. Adams, corresponding secretary of the Academy, and signed A. Bradford, inclosing various information from individuals who had seen the serpent. He states in his letter, as within the recollection of many, that various vague reports had been circulated, in the course of a few years, of an animal of this description having been seen in Penobscot Bay, to which, however, but little attention was paid; but happening to hear that one was seen in that bay in 1802, he instituted inquiries to ascertain the truth of the report. This produced letters from the Rev. Mr. Cummings of Sullivan, and the Rev. Mr. M'Lean. He afterwards learnt that George Little, esq. late commander of the Boston frigate, saw a similar sea monster in the time of the revolutionary war, and on writing to him on the subject he had the fact confirmed. He also inclosed a letter from a Capt. Crabtree of Portland, who had at another time seen one of these serpents.

In July, 1802, Mr. Cummings saw this extraordinary sea monster in his passage to Belfast, between Cape Rosoi and Long Island. He took it at first for a large shoal of fish with a seal at one end of it; but as he drew near the boat, those on board saw that this whole appearance formed but one animal: it seemed to have an ascending and descending serpentine motion. The people in the boat judged the length to be more than 60 feet: it had "a serpent's head of a colour as blue as possible, and a black ring round its eyes. The head was three feet in circumference *at least*.....Two young men on Fox Island, intelligent and credible, saw an animal of this kind about 5 years since [*i.e.* early in 1799]. They told me the one they saw was about 60 feet long, and appeared to have an ascending and descending motion. A few years before, perhaps 10 years [about 1794,] two of those large serpents were seen by two other persons on that island.

About

About 20 years since, [about 1804,] two of them were seen by one Mr. Crocket, who then lived upon Ash Point.

Mr. M'Lean states that a sea serpent was seen by the deceased Capt. Paul Reed of Boothbay, about 30 years before 1804; and another was seen in Muscongus Bay in the time of the American war, two miles from the place where he then lived; and another soon afterwards off Meduncook.

Capt. Little states, that in May 1780 he was lying in Round Pond, in Broad Bay, in a public armed ship, and at sunrise discovered a large serpent, or monster, coming down the bay, on the surface of the water. "The cutter was manned and armed. I went myself in the boat (says Capt. Little), and proceeded after the serpent. When within a hundred feet, the marines were ordered to fire, but before they could make ready the serpent dove." Judged to be from 45 to 50 feet long; largest diameter of body about 15 inches: head about the size of a man's, and carried from 4 to 5 feet out of the water—had every appearance of a common black snake. "When he dove he came up near Muscongus Island—we pursued him, but never came up within a quarter of a mile of him again." . . . . "A monster of the same description was seen in the same place by Joseph Kent of Marshfield, 1751. Kent said he was longer and larger than the boom of his sloop, which was 85 tons. He had a fair opportunity of viewing him, as he was within 10 or 12 yards of his sloop."

Capt. Crabtree, then of Fox Island, in the Bay of Penobscot, being informed by a neighbour that a large sea serpent was on the water near the shore, just below his house, and having before heard stories of the same kind, which he discredited, went to satisfy himself, and "saw a large animal in form of a snake lying almost motionless in the sea, about 30 rods from where he stood" —his head about 4 feet above water—apparent length 100 feet —diameter about 3 feet. He also states, that before that time many people living on these islands declared to him that they had seen such an animal.

So far from the communication to Mr. Adams in 1804. These statements Professor Bigelow follows up by other statements, most of which having appeared in the newspapers, we shall notice them very briefly:

Capt. Perkins saw a monster of this description at Gloucester in 1817.

On the 6th of June, 1819, Captain Wheeler, then in his sloop Concord sailing from New York to Salem, fourteen miles west of Race Point, about five in the morning, saw a sea snake directly a head, about 100 yards from the sloop, moving in a S.W. direction, which it kept till it passed athwart the course of the vessel, and appeared directly over the weather bow, when he

he altered his course to S.E. After being seen about five minutes it sunk, and in about 8 minutes after appeared again directly over the weather quarter, about the same distance from the sloop, and in about six minutes more he sunk and did not rise again.—Had a distinct view of the creature: it was entirely black; the head, which resembled a snake's, was elevated from four to seven feet above the water, and his back appeared to be composed of bunches or humps, apparently as large or larger than a half barrel. Tail not seen, but from head to last hump apparently about 50 feet in length.—Capt. Wheeler's statement is on oath.

At 7 o'clock the same morning, G. Bennett, the mate of the foregoing sloop, had his attention called to something alongside by the man at the helm: it was the same serpent, or one similar to that seen by those on deck two hours before. It was not more than 14 rods from the vessel: its head was about seven feet out of the water: it was black, and the skin seemingly smooth, without scales; the head as long as a horse's, but "a proper snake's head"—there was a degree of flatness, with a slight hollow on the top of his head—the eyes prominent, and standing out considerably from the surface like those of a toad, and nearer to the mouth than to the back of the head. The back composed of bunches about the size of a flour barrel, and three feet apart—they appeared to be fixed, but this might be occasioned by the motion of the animal, and looked like a string of casks tied together. The tail not visible, but it showed a horizontal or sweeping motion, producing a wake as large as the vessel made. The part visible appeared to be about 50 feet in length. While the mate was ascending the rigging to get a better view, the animal sunk and did not rise again.—This account is also on oath.

On the 13th of August, 1819, a sea serpent was seen near the Long Beach of Nahant, by James Prince, marshal of the district, and more than 200 persons. It had been seen the evening before at Nahant beach by many people from Lynn. It had the general appearance already described—the bunches on his back were 13 to 15—from 50 to 60 feet in length. Mr. Prince had more than a dozen distinct views of him with a good telescope from the Long Beach, and at some of them the animal was not more than 100 yards distant. It was seen at intervals from a quarter past eight till half-past 11 in the morning—the water quite smooth.

Mr. Samuel Cabot gives a similar description of the serpent seen the 13th of Aug. 1819.

Mr. Cheever Felch, chaplain of the United States' ship Independence, of 74 guns, also describes the sea serpent as seen from the

the United States' schooner Science, off Gloucester, on the 19th of Aug. 1819, within 20 yards of him. They followed him for some time, and saw him very distinctly. He describes it as dark brown, with white under the throat. Could not ascertain the size, but the head appeared to be about three feet in circumference, flat, and much smaller than the body. Did not see his tail, but from the head to the end of the part seen was about 100 feet. Mr. Felch counted 14 bunches on his back, the first one 10 or 12 feet from his head, and the others about seven feet apart.

Pontoppidan's account of the *Serpens marinus magnus* in his History of Norway, published in 1747, seems to describe the same kind of sea serpent seen on the American coast. "It is usually seen in July and August, and when it is calm—'His head was more than two feet above the water, and resembled that of a horse. Beside the head and the neck, seven or eight folds or coils of the animal were distinctly seen, and were about a fathom apart.' This is the statement of a Capt. de Ferry and others, who saw the sea serpent with him. Others state that when it was calm it lay on the water in many folds; and that there were to be seen above the water small parts of the back when it moves or bends; and that at a distance these appear like so many casks or hogsheads, floating in a line, with a considerable distance between each of them."—The historian adds "that many other persons on the coast of Norway had seen the sea serpent—and thought it a strange question, when *seriously* asked, whether there was such an animal in existence; being as fully persuaded of the fact as of the existence of an eel or a cod."

LXI. *Third Report of the Commissioners appointed by His Majesty to consider the Subject of Weights and Measures.*

May it please Your Majesty,

We, the commissioners appointed by Your Majesty for the purpose of considering the subject of weights and measures, have now completed the examination of the standards which we have thought it necessary to compare. The measurements which we have lately performed upon the apparatus employed by the late Sir George Shuckburgh Evelyn, have enabled us to determine with sufficient precision the weight of a given bulk of water, with a view to the fixing the magnitude of the standard of weight; that of length being already determined by the experiments related in our former reports: and we have found by the computations, which will be detailed in the Appendix, that the weight of a cubic inch of distilled water, at 62 deg. of Fahrenheit, is 252·72 grains of the parliamentary standard pound of 1758, supposing it to be weighed in a vacuum. We

We beg leave therefore finally to recommend, with all humility, to Your Majesty, the adoption of the regulations and modifications suggested in our former reports, which are principally these:

1. That the Parliamentary standard yard, made by Bird in 1760, be henceforward considered as the authentic legal standard of the British empire; and that it be identified by declaring that 39,1393 inches of this standard, at the temperature of 62° of Fahrenheit, have been found equal to the length of a pendulum supposed to vibrate seconds in London, on the level of the sea, and in a vacuum.

2. That the Parliamentary standard Troy pound, according to the two-pound weight made in 1758, remain unaltered; and that 7000 Troy grains be declared to constitute an Avoirdupois pound; the cubic inch of distilled water being found to weigh at 62 deg. in a vacuum, 252·72 parliamentary grains.

3. That the ale and corn gallon be restored to their original quality, by taking, for the statutable common gallon of the British Empire, a mean value, such that a gallon of common water may weigh 10 pounds avoirdupois in ordinary circumstances, its content being nearly 277·3 cubic inches; and that correct standards of this imperial gallon, and of the bushel, peck, quart, and pint, derived from it, and of their parts, be procured without delay for the Exchequer, and for such other offices in Your Majesty's dominions as may be judged most convenient for the ready use of Your Majesty's subjects.

4. Whether any further legislative enactments are required, for enforcing a uniformity of practice throughout the British empire, we do not feel ourselves competent to determine: but it appears to us, that nothing would be more conducive to the attainment of this end, than to increase, as far as possible, the facility of a ready recurrence to the legal standards, which we apprehend to be in a great measure attainable by the means that we have recommended. It would also, in all probability, be of advantage to give a greater degree of publicity to the appendix of our last report, containing a comparison of the customary measures employed throughout the country.

5. We are not aware that any further services remain for us to perform, in the execution of the commands laid upon us by Your Majesty's commission: but if any superintendence of the regulations to be adopted were thought necessary, we should still be ready to undertake such inspections and examinations as might be required for the complete attainment of the objects in question.

(Signed) George Clerk. Thomas Young.
Davies Gilbert. Henry Kater.
Wm. H. Wollaston.

London, March 31, 1821.

LXII. *A new Method of teaching Latin to Youth; by* Robert John Thornton, *M.D.*

To Mr. Tilloch.

Sir, — An explanation of the *new method* I have adopted for communicating a knowledge of the *Latin language*, appears to me not to be unsuited to your Philosophical Magazine. I was myself educated at a public school, in the usual mode, afterwards of Trinity College, Cambridge; and having a son at Westminster, I became fully convinced that there required some other method of teaching Latin than that at present pursued in our several places of education. I therefore have investigated in my preface to the "*Pastorals of Virgil*," which I published, all the different modes invented for making youth acquainted with the Latin tongue; and I think I have demonstrated, that all the several modes used generally fail of the intended purpose. I shall now first consider the *interpretatio* and *ordo*. This forms that part of facility given to boys in the Delphin Virgil, and the Delphin classics in general. Now I believe, that nothing tends so much to mislead, and does a greater injury to boys than this *interpretatio* and *ordo*, which is in constant use in almost every school. I communicated this opinion to a very high dignitary of the church, one of the best classical scholars of the age, and he agreed in this most cordially with me, and calls my publicly reprobating such a scheme "*most meritorious.*" This opinion other great classical scholars, and men deeply versed in the education of youth, also confirm. Thus the learned Rev. Dr. Trollope, Master of Christ Hospital, concurs with me, and writes that I have deserved well of the community by omitting the *interpretatio* and *ordo*. In the English, our words follow each other in the sense to be conveyed, and this holds in all northern languages. The words are short and abrupt, and not fitted so much for eloquence as force. Whereas in warm climates, people delighting to be out of doors, hence the *ore rotundo*, the long flowing syllables, and fine turned periods. Even in common prose in Xenophon, we have a simple word of one-and-twenty syllables, in the *Anabasis*. By the peculiar construction of the Greek and Latin language the attention is kept up, and every single word is placed in that very position best adapted for it. It is like a building, where the displacement of any part would mar the whole. How barbarous, then, the making the Latin words correspond to our mode of English expression! It is as absurd as the Gothic custom of cutting trees into the forms of peacocks and other animals, instead of having their native shapes. I hold that no one word in Virgil is faulty, or could be

be displaced for another. Each has its precise meaning, as well as place: what shall we say, then, to not only a disarrangement of position, but also a substitution of one word for another? proving either that Virgil did not know his own language, or, rather, attempting to adopt other expressions more easily convertible into our modern languages. This is like dressing a Roman in a bag-wig and sword, and putting over him a modern *costume.*

I early made my son feel, that the great perfection of Virgil was, that he had the art of saying the best things in the best manner, and that all alteration was disfigurement.

This distortion then, to make boys understand Virgil, is therefore the sure method of making them insensible to his beauties, to the propriety of his diction, and the harmony of his periods. The mock imitation of him occasions his real person to vanish, and we become contented with a shadow. Those words which burn in verse, sink into lifeless prose; and what is worse, when the taste once becomes corrupted, it cannot easily be relighted; and the boy accustomed to such a bad facility, does not afterwards readily apply himself to catch the true meaning from the author himself. Having discarded, therefore, this facility, a question now arises, What other facility, or facilities, shall be substituted?

If we open any book even in English, not knowing what has preceded, nor able to guess at what is to follow, the meaning of any sentence so taken up, is difficult at once correctly to make out. This is more particularly the case in Latin. A *clue* therefore should be given, to enable the reader to know what he is to expect. If this be a *literal translation* (used in some schools), the boy so instructed anglicises his Latin, if I may so express myself. He gets not the Roman conception of Latin; he feels not the energy of that fine language, so superior to modern tongues; nor is he alive to that power of position arising from the repeated changes in termination, governed by certain philosophical rules denominated grammar. If the *translation* be *loose* (adopted in some schools), the boy, being blind himself, has "a blind guide, and both fall into the ditch." He is always at sea, and catching at aids which, like the *will of the wisp*, are sure to lead him into a wrong path. We have, therefore, also abandoned these two bad facilities, and supplied in their place, only *an introduction* or *previous comment*, to make the discovery of the true meaning of the author more easily intelligible in his own language.

Notes form the next point to be considered. These are wanted in all languages, and more particularly in the dead, where allusions to foreign customs are constantly made. The Delphin notes are many of them extremely good; but as the Latinity we

should

should learn, should be from the classics themselves, and being in a foreign language are not easily understood; this facility should be granted in our own native tongue, and then they will be read over and over again by the diligent youth, and he will not be too long impeded in the comprehension of his author. The notes in our Virgil are therefore selected from all the several commentators, and given in English, and many additional ones are added. They are numerous, or otherwise Virgil would not be clearly understood. As the Greeks were themselves the precursors of the Romans in all that was excellent, and the latter did not think it a plagiarism to transfuse any brilliant idea from these masters in science; but, on the contrary, thought they approached nearer to perfection, the more they illumed their torch from the divine fire of these godlike men: and like a great painter, who shows he is a disciple of some old master; so Virgil does not translate the Idyllia or Pastorals of Theocritus; but catches at his spirit and fire, and here and there dashes in the very words of that divine poet; and shows he was not ashamed to acknowledge the cradle in which his muse was nursed.

To understand the skill of Virgil, therefore, in this particular, it became necessary, that those Greek Pastorals which have any reference to Virgil, should be either previously read in the original, or in a translation. Unfortunately the custom has prevailed, although the Scriptures are in Greek, to teach the Latin first; and this, I suppose, arose from the absurd practice, now getting gradually abolished, of all our Lexicons being in the Latin tongue. It behoved us, therefore, to give *translations* of such of the Idyls of Theocritus as have any reference to those of Virgil, which are made as *introductory* matter to several of the Pastorals of Virgil, and which show to youth, how far it is allowable for a great mind to copy from a precursor. After this knowledge, as our first poets have transfused into their pastorals many of the leading incidents and expressions of Virgil, to feel the obligations the moderns owe to the ancients, as well as for recreation and pleasure, select *modern poetry* is added, which cannot fail to expand the germ of genius, and make it blossom and bear fruit in due season. As youth is the period most adapted for impressions, (for, as Locke says, "the mind is a *rasa tabula*, on which you may write either God or Devil,") a *Moral* is also added after each Eclogue, which appeared to arise out of the subject; and thus youth are put into possession of not only a *Latin book*, but also an *English book*, which seem naturally to connect themselves together; and which obviates the objection often made, that the student is a mere *book-worm*, only conversant with large dusty folios; for by this method he is early initiated into the beauties of our greatest English poets. To render this work yet more

 serviceable

serviceable and alluring to youth, upwards of two hundred and thirty beautiful *wood-cuts* are added, which, as being unexplained, are sought after in the Latin, and provoke curiosity, as well as elucidate. Such a plan has, I am happy to say, obtained the favourable approbation of the first scholars of the age; and our Pastorals of Virgil have now passed through a third edition, and seem likely to be adopted by school-masters in general *.

Claim to the Invention of a new Method of determining the Latitude. By Mr. EDWARD RIDDLE.

To Mr. Tilloch.

SIR,—On the appearance of the last part of the Transactions of the Royal Society of Edinburgh, I observed a communication from General Brisbane, respecting a method of determining the time, (which he of course conceived was either not generally or not sufficiently known,) accompanied by a promise to transmit to the Society an account of a method of determining the latitude by observations made near noon. The method of determining the time which he detailed in that memoir, was exactly the same as one which I had practised for a considerable period; and I thought it, therefore, probable that his promised method of determining the latitude might also be similar to that which I was daily in the habit of practising.

That a fair opportunity might be afforded of comparing our methods, I immediately addressed a letter to you on the subject, containing a detailed account both of the principles of my method, and of the manner in which the observations and calculations were made; and I added an example, with the calculations at length. The observations for the example were taken, and the calculations completed in the form in which they were sent to you, in the year 1817. My letter, I find, is dated October 21, 1818, and it was printed in the Philosophical Magazine for December the same year.

General Brisbane's method is now before the public. It forms Art. XIV. Vol. IX. Part I. of the Edinburgh Phil. Trans., just published; and it appears to have been read November 20, 1820. The observations which are given as examples were made in February 1820, more than a year after my letter on the subject had been published in the Philosophical Magazine; and more

* We annex a Plate, to show the nature and value of the wood-cuts in "*Thornton's Pastorals of Virgil*, in which all the proper facilities are given, enabling youth to acquire the Latin language in the shortest period of time, with the utmost delight to the scholar, and ease to the master."—EDITOR.

than

than two years after the observations given in that letter were made and computed from.

The method of Gen. B. is even more like mine than I was likely to anticipate, as *it is absolutely the same, both in principle, and in all its practical details.* The only difference, if a difference it can be called, is in this, that I deduce the latitude each day from separate observations of the upper and lower limbs of the sun, taken alternately; by which means any faulty habit of estimating the contact is prevented from vitiating the result. In Gen. B.'s two examples this precaution is not observed; and it is easy to perceive that, in consequence, each of his results *may* be affected by a common error incident to his method of estimating the contact. Setting this aside, however, as a circumstance from which, in the case of so excellent an observer, no error of any moment whatever will probably arise, the absolute identity of his method and mine is beyond all question.

Gen. B. computes the declination for the instant of each observation, so do I; he reduces each altitude to the meridian separately, so do I; he deduces the latitude from each altitude thus reduced separately, so also do I; and the theorem by which he calculates the reduction to the meridian is the same as that which I use for the same purpose. His theorem is f (the reduction) $= \frac{2 \sin^2 \frac{1}{2} P}{1''} \cdot \left(\frac{\cos H \cdot \cos D}{\cos (H \pm D)}\right)$; where H is the latitude, D the declination, and P the horary angle.

My theorem is x (the reduction) $= \frac{\text{vers } P}{1''} \cdot \sin PS \cdot \sin PZ \cdot$ cosect ZS; where PS = the polar distance, PZ = the colat., and ZS = the meridian zenith distance. Now vers $P = 2 \sin^2 \frac{1}{2} P$, $\sin PS = \cos H$, $\sin PZ = \cos D$, and cosect $ZS = \frac{1}{\sin (H \pm D)}$. The theorems are therefore the same; only the one which I used is a little more convenient in form. But on the subject of the *theorem* I dare say neither of us fancies that he has made any discovery.

Though the absolute identity of the two methods *in every particular* is, to say nothing else, a very curious circumstance; I have no reason to believe that Gen. B. has availed himself of any thing that I have done on the subject, notwithstanding the publication of my letter took place, probably, long before his communication was written. I understand he has of late chiefly resided abroad; and even when in this country he probably may not see the work in which my letter appeared.

But I cannot help thinking it exceedingly singular, that his paper should have been printed in so respectable a publication, without a hint from any one connected with the work, that however

ever creditable the memoir might be to its author, it had not to the public the recommendation of containing any thing *new*. And my surprise is by no means diminished by the reflection that several of Dr. BREWSTER's publications bear testimony to his being a constant reader of The Philosophical Magazine.

I am, sir, respectfully,
Your obedient servant,
EDWARD RIDDLE.

Trinity House School, Newcastle,
May 14, 1821.

LXIV. *Experiments on the Strength and Stiffness and Specific Gravity of various Specimens of Wood. Extracted from the "First Report from the Select Committee appointed to consider of the Means of improving and maintaining the Foreign Trade of the Country**.

Feb. 13, 1821.

JOHN WHITE, Esq. a gentleman engaged to a considerable extent in the timber trade, was called in and examined.

"You have been engaged in a course of some experiments, to ascertain the strength and value of different species of wood?—I have been so for the last two or three years.

"Have the goodness to state the result of those experiments. —I think a short extract of the experiments which I have made, will probably give that information. Mr. Lack wrote me a note, saying it was possible I might be called upon for the information.

The witness read the statement as follows:

"Results of Experiments on the Stiffness and Strengths of various Specimens of Wood.

"The trials were made upon pieces carefully selected as to quality and grain, and were, in substance, two feet long, one inch square; they were all from split portions of timber. The order of stiffness was,

"No. 1.	Long Sound timber, bent half inch in the middle by ..	261 lbs. avoirdupois.
2.	Christiana white spruce fir ..	261
3.	English oak, young wood; suppose 60 years; from King's Langley, Herts	237
4.	American pine, yellow or soft; from Quebec	237
5.	Riga oak (commonly called wainscot)	233
6.	White spruce from Quebec ..	180
7.	English oak from Godalmin, suppose 200 years; old timber	103

* Ordered to be printed 9th March 1821.

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"The order of Strength, as ascertained by their being broken by the application of weight, was,

1.	English oak, King's Langley	482 lbs. avoirdupois.
2.	Long Sound yellow fir ..	396
3.	Riga oak (wainscot)	357
4.	Christiana white spruce ..	343
5.	American pine, from Quebec	329
6.	White spruce fir, from Quebec	285
7.	English oak, from Godalmin	218

"Other trials of Strength were as follows:

1.	Alice Holt forest, full grown timber No. 1	455 lbs. avoirdupois.
2.	Dantzic fir, yellow	435
3.	Alice Holt forest, full grown timber, No. 2	405
4.	Christiana yellow fir	370
5.	Archangel, ditto .	330."

From the manner in which the foregoing statement is given in the Report, most people would be apt to conclude that these experiments were actually made by Mr. White himself; whereas, in fact, they were made by Mr. Tredgold, and published in the year 1820, in pp. 34, 35 and 44, of his *Elementary Principles of Carpentry*. We understand that all the claim which Mr. White has to these experiments is his having furnished Mr. Tredgold with the specimens.

The following is copied from the Appendix.

"Navy Office, 19th Feb. 1821.

"An Account of the Specific Gravity, Strength and Deflection, of the several Kinds of Foreign Fir, as found by Mr. Peter Barlow, by Experiments made under his Inspection, from Timber supplied from His Majesty's Yard at Woolwich.

"N. B. The pieces tried, were eight feet long, two inches square, supported by props seven feet apart, and had the weights placed in the middle of each piece.

Names of the Woods.	Average Specific Gravity.	Breaking Weight.	Ultimate Deflection.
		lbs.	in.
American red pine	657	511	5·82
New England fir, or yellow pine ..	553	420	4·66
Riga fir	753	422	6·00
Norway spar	577	655	4·00

"These experiments do not appear to have been extended to Dantzic

Dantzic fir; but, from the best judgement I can form, the Dantzic and Riga timber assimilate in quality and strength, when equally clear of knots. "RT. SEPPINGS."

The evidence of Lancelot Holland, Esq. presents some curious facts. The following is an extract:

"In what business are you engaged?—A timber merchant.

"Are you also a builder?—No, I have never been a builder.

"You are acquainted with the timber of the north of Europe and of North America?—Buying and selling considerably all foreign timber.

"Can you speak to the comparative quality of the different kinds of timber?—The best timber which comes into London, I believe to be the Riga timber. The Dantzic timber is a larger grown timber; it is of a size which you cannot procure from Riga, and the quality is perhaps quite as good. I think the general prejudice is in favour of the Riga; and that next to those comes the Memel, which is of exactly the same size as the Riga timber, but it is coarser; it is more knotty, and therefore more liable to break. Then comes the Swedish timber; next to that, the red pine timber from America. I omitted Norway timber because there is so very small a quantity of it used; the duty is so very heavy; there is hardly any of it imported to make it a subject of consideration; the red pine timber is about equal in price, and I believe as good as the Swedish timber in quality.

"Where would you place the Norway timber in point of quality?—Perhaps first, certainly not second to any. After the red pine, you come into a large field of different timbers, which come from America, which are all called the yellow pine, and are all of an inferior quality, of a softer wood.

"What are the differences?—I think that shipped from Miramichi is rather stronger than what comes from Quebec.

"You consider the American timber as greatly inferior to the Baltic?—Not the red pine. I think, if the red pine is well manufactured, it is as good as any timber that comes into London. I believe, if the red pine could be manufactured so as to be exactly like Riga timber, there would not be more than 10*s*. a load, I am sure not more than 5*s*. difference between the value of that and Memel timber; but in the manufactory, it is to be considered, the timber is *all* measured here, and if it is not quite square and parallel there is a great loss, the angles being measured as if they were square; this loss might amount to about 10 per cent.

"Do you consider the yellow timber as very inferior?—That is not fit for building purposes; it is very good for some purposes; for finishing the inside of rooms it will do, but it will not do for strength;

strength; it is a much weaker timber. I speak both of the Quebec and the Miramichi.

"In point of durability, how do you rank them?—That is according to the purpose; this table will last if it is made of either; but if you put the American in damp situations, it will perish more rapidly than any other.

"Is it more liable to the dry-rot?—All wood is liable to the dry-rot; but I think the soil is more congenial to the growth of the dry-rot in yellow pine; the wood is more obnoxious to it.

"Much more than in the red pine?—Much more than any other timber.

"Is the red pine more liable to it?—I apprehend it is not more so than Swedish timber.

"Do you apprehend the length of the voyage from America contributes to the dry-rot?—If wood is put on board a ship in a damp state, which it is nine times in ten, you will see a tendency to dry-rot whenever the timber is delivered; that is, you will see the commencement of the dry-rot on the surface.

"By wet do you mean green?—Not entirely; in Memel it is kept under water, when they wish to preserve it, for a great length of time; they consider that will preserve it better than an alternate exposure to wet and dry.

"Have you observed this more in the American timber than in the timber from the Baltic?—The voyage being longer, the dry-rot has had more time to grow; but if it is shipped in a dry condition, it will come out perfectly good and sound.

"Do you know the different purposes to which the different kinds of timber are generally applied?—The first timbers I mentioned are applied for what is called builders' purposes, that is to say, for the frame work of a house; then the yellow pine is not at all applicable to that purpose; the red pine is.

"To what purposes is the yellow pine applicable?—The yellow pine is used by coach-makers, blind-makers; it is better for musical-instrument makers than any other; for mouldings, where you want soft wood to carve picture frames; chests and packing-cases, to an immense amount.

"What proportion of the wood imported into this country is employed for those purposes?—It would astonish the Committee to know the quantity used for packing-cases in London alone; if If I were to guess, I should say one-fourth of the whole wood that comes into London.

"For those inferior purposes this description of wood is as good as the superior description would be?—No, not for the packing-cases altogether; a great number of the packing-cases made for long voyages they are obliged to make out of Baltic deals, because the American is so very soft it will not hold the nails; the East

India Company have perhaps half and half, or rather more Baltic than half; they will not admit it except for copper cases; for their stationery, and other things they are very anxious to preserve, where the splitting of wood would be of consequence, they will not admit the American.

"Do they use the Baltic timber now for many of those purposes?—The Baltic deals are used for packing-cases to a very great extent; the price is not very different."

LXV. *Some Account of the principal modern Catalogues of fixed Stars; with Remarks connected with the Subject.* *By* A CORRESPONDENT.

"Celui qui cherchoit à se procurer des Tables Astronomiques, pouvoit d'abord être, avec raison, embarrassé à choisir les meilleures et les plus exactes; ensuite il ne pouvoit acquérir celles qui lui étoient nécessaires, sans beaucoup de peine, de temps, et de dépense." "Un Astronome est donc obligé d'acheter une bibliothèque, tandis qu'il ne demandoit qu'un recueil choisi de Tables. C'est pourquoi l'on souhaite depuis longtems que les Astronomes publient les résultats de leur travaux d'une façon plus commode et plus aisée." — *Préface aux Tables de Berlin, par Bode, Lambert, et Schulze,* 1776.

IT has long been a matter of regret with many persons, who, though not professed astronomers, are in the habit of devoting their leisure moments to the cultivation of celestial science; that there exists not any catalogue of stars, the production of a British press, upon which any reliance can be placed at the present day, either for magnitudes or positions; whilst the foreign publications of the kind are in general expensive and of difficult access, and some of them are so to a very great degree.

The above quoted remarks are therefore equally applicable now, as at the period when they were originally made, at least with regard to the Tables of the fixed Stars; since the lapse of 45 years has rendered entirely obsolete the Collection of Tables, in three volumes, octavo, already referred to, which had been assembled together by the laudable industry of the three distinguished editors, under the patronage of the Royal Prussian Academy.

Wollaston's "Specimen" (as the author modestly termed it) published 32 years since, was received by astronomers with considerable eagerness; not because the materials employed in its compilation possessed the wished-for accuracy, but because it presented those materials in a connected and systematic form. The author in his preface insisted strongly on the necessity of astronomers acting in concert, in order to remove those defects which his own labours had exhibited in the most prominent manner.

ner. His exhortations seem to have produced the desired effect; for, from that time, observations on the fixed stars were multiplied to a surprising extent, so as to exceed, in a few years, all that had been done for a century past, as will hereafter more distinctly appear.

The results have been printed from time to time in a considerable number of bulky volumes; and since all our astronomical works are very brief and inaccurate in their accounts of these publications, I apprehend the subjoined sketch of the principal ones may not be unacceptable, taking them in chronological order.

1. "A Specimen of a General Astronomical Catalogue, arranged in Zones of North Polar Distance, and adapted to Jan. 1, 1790; containing a comparative view of the mean positions of Stars, Nebulæ, and Clusters of Stars, as they come out upon calculation from the tables of several principal observers; together with a proposal for setting on foot some regular method of observing the heavens, through the concurrent assistance of astronomers in all nations, in order to form a more perfect register of their present state, and discover any alterations to which they may regularly be subject, or which they may at any time hereafter undergo. By Francis Wollaston, F.R.S." Fol. London, 1789.

I place this work at the head of the list, although little reliance can be placed upon it either for the positions or the magnitudes of stars; because it is the most recent British publication (except the quarto work, No. 3); and as the arrangement differs from that of any other, it may occasionally be useful to persons who adopt a peculiar method of observing. I shall content myself with referring for a minute account of the book, to the Monthly Review, New Series, vol. ii. p. 306, or to Rees's *Cyclopædia*, article CATALOGUE.

2. "Catalogue of Stars taken from Mr. Flamsteed's observations contained in the second volume of the *Historia Cœlestis*, and not inserted in the British Catalogue; with an Index to point out every observation in that volume belonging to stars of the British Catalogue. To which is added, a collection of errata that should be noticed in the same volume. By Carolina Herschel. With introductory and explanatory remarks to each of them by Wm. Herschel, LL.D. F.R.S." Fol. London, 1798.

This work is an indispensable companion to the preceding, as well as to the original work of Flamsteed. Besides giving the positions of near 400 additional stars observed by Flamsteed, it furnishes many important emendations of that author's catalogue. The Index of Observations may frequently be referred to with

 advantage,

advantage, even by those who are not possessed of the *Historia Cœlestis* *.

3. "*Fasciculus Astronomicus*, containing Observations of the Northern Circumpolar Region; together with some account of the instrument with which they were made: and a new set of Tables, by which they were reduced to the mean position for the beginning of January 1800. To which are added a few other papers and precepts which it was imagined might be acceptable to the practical Astronomer. By Francis Wollaston, F.R.S." 4to. London, 1800.

The circumpolar zones being the most defective part of his general catalogue, the editor undertook a review of that region, as his own share of the proposed general plan. The volume now under consideration contains the observations at large, with a catalogue thence resulting, exhibiting the mean places of about 260 stars, all within 26° of the pole. In publishing the positions of the greater part of these stars, Dr. Wollaston was anticipated by the two Lalandes, whose cotemporary observations of 1000 circumpolar stars were printed in the *Connaissance des Tems*, An. V. (1797), and are incorporated in Bode's work, to be presently noticed. At the end of the book are a considerable number of corrections to be made in the author's General Catalogue.

4. "Histoire Céleste Françoise, contenant les Observations faites par plusieurs Astronomes François: Publiée par Jerome de la Lande." 4to. Paris, 1801.

The chief feature of this work consists in a systematic series of observations of 50,000 stars, made with a mural quadrant by the editor, in conjunction with his nephew Michaël Lefrançois Lalande, between the years 1789 and 1801. They comprise stars of all magnitudes, down to the 9th inclusive, and extending 78° from the zenith of Paris towards the South. There are also a great number of observations to the North of the zenith; but to complete the polar region, recourse must be had to a series of observations of earlier date, inserted in the Memoirs of the Academy of Sciences for 1789 and 1790. The mean positions of 12,000, selected from the above 50,000, and reduced by Madame Lefr. Lalande, have been published by successive portions in the *Connaissance des Tems*, An VII–XIII. inclusive. Among the stars so published, are found very few whose positions had been previously determined. The stars of the 8th and 9th magnitudes remain for the most part unreduced.

* It may not be improper to mention, that besides the observation of Herschel's planet, made by Flamsteed, which produced 34 *Tauri*; this work contains *five* other observations, that have lately been ascertained to belong to the planet, viz. Nos. 349, 365, 366.—See *Conn. des Tems*, 1820, p. 408.

5. "De-

5. "Description et Connaissance Générale des Constellations, avec un Catalogue de l'Ascension droite et de la Déclinaison de 17,240 Etoiles, doubles, nébuleuses, et amas d'étoiles, par J. E. Bode." (German and French.) Fol. Berlin, 1801.

Being (like Wollaston's) a compilation from the whole of the materials of which Astronomers were then in possession, this work sufficiently indicates the rapid progress that had been made in the course of 10 or 12 years towards supplying the deficiencies of the other. The arrangement is totally different from that of Wollaston, the stars, &c. being classed in Constellations, the number of which is augmented to 102. Of the chief materials employed in its compilation, I shall give an account in the author's own words.

"J'ai commencé par les étoiles du Catalogue Britannique de Flamsteed, j'y ai joint ensuite toutes celles que j'ai trouvées ailleurs, chez Hevel, T. Mayer, de la Caille, Messier, Mechain, Bradley, Darquier, de la Lande, Herschel et autres. J'ai recherché chaque fois, autant qu'il m'étoit possible, les observations les plus récentes, et, comme on peut croire, les plus exactes, et je n'ai conservé, par exemple, des étoiles de Flamsteed, que la place de celles qui ne sont désignés par aucun autre astronome. Mais ma collection doit surtout un riche accroissement à l'amitié obligeante de Mr. de la Lande, qui m'a envoyé à plusieurs reprises, soit en manuscrit, soit en feuilles à mesure qu'ils paroissoient, le catalogue de plusieurs milliers d'étoiles, tirées des années 1799–1802 de la *Connaissance des Tems*, et nouvellement observées à Paris, pour le plupart par Mr. le François son neveu. De plus, j'ai calculé la place de plus de 2400 étoiles, d'après les observations que M. de la Caille a faites au Cap de Bonne Esperance, et je les ai jointes à son catalogue connu de 1942 étoiles méridionales. Outre cela, les observations très exactes que Mr. Vidal a faites tout récemment sur un grand nombre d'étoiles méridionales, me furent communiquées par la complaisance de Mr. de la Lande. Enfin, j'ai observé encore moimême à l'observatoire de Berlin, depuis le mois de Mars 1797 jusqu'en Decembre 1799, au delà de 1250 étoiles, qui pour la plupart ne se trouvent encore dans aucun catalogue connu.

"J'ai calculé l'ascension droite, et la déclinaison, des nébuleuses d'Herschel, de ses doubles, de ses amas, &c. d'après l'indication qu'il donne de leur différentes positions relativement à des étoiles connues de Flamsteed*."

The stars, &c. in each constellation are arranged in the order of Right Ascension, with a regular numerical series attached to

* Sir W. Herschel's *third* catalogue of nebulæ and clusters (Phil. Trans. 1802, Part II.) is of course not comprised in the above work of Bode.

each.

each. The most comprehensive is that of Virgo, which contains 736 celestial objects. The entire catalogue is printed in so compact a form, as to occupy no more than 96 pages.

6. " Præcipuarum Stellarum inerrantium positiones mediæ, ineunte seculo XIX. ex observationibus habitis in speculâ Panormitanâ, ab anno 1792 ad annum 1802." Fol. Panormi, 1803.
7. " Præcipuarum Stellarum," &c. (ut suprà) " ab anno 1792 ad annum 1813." 4to. Panormi, 1814.

Notwithstanding the great extent and utility of Bode's Catalogue, yet too many positions therein (particularly among stars of the 4th, 5th, and 6th magnitudes) were inserted merely from the authority of Flamsteed and other old observers, to give entire satisfaction to astronomers. They therefore received with eagerness the new catalogue of 6748 stars, presented by Professor Piazzi, as being entirely founded on observations made at the same place and with the same instruments. The stars are arranged in it, in one continued series, following the order of Right Ascension: and as the author had taken Wollaston's Catalogue for his guide, there are few stars in that work whose positions he had not examined and corrected*.

After a lapse of eleven years, a second edition made its appearance, founded on a far greater number of observations. " In this new catalogue, in which the number of stars is 7646, M. Piazzi has not chosen to adopt any thing which he had not himself verified. He determined the right ascensions of the fundamental stars by a direct comparison with the sun. The others have been deduced, as usual, by the difference of their passages over the meridian, observed a great number of times, and the mean result has been taken." The number of observations from which each position has been deduced, is inserted in a proper column: they are generally seven or eight, and frequently extend to double the number. The proper motions are also given " whenever it was possible to find in the earlier catalogues positions sufficiently exact for the purpose of comparison. The notes which accompany this catalogue offer many curious remarks on the stars whose motions had not yet been observed, or of which the brilliancy appears to vary periodically."

* Professor Bode reprinted this first edition of Piazzi in an abridged form, in small quarto, containing 5505 stars; and annexed it to the second edition of his small Atlas. The work is entitled " Représentation des Astres sur 34 planches en taille douce, avec une instruction sur la manière de s'en servir, et un catalogue de 5877 étoiles, nébuleuses, et amas d'étoiles, par J. E. Bode." Berlin et Stralsund, 1805. This catalogue is perhaps the best that can be recommended to the young astronomer for general purposes, particularly as it is more easily accessible than either of the original editions. The want of notes, and of proper synonymes, may be mentioned as its chief defect.

Important,

Important, however, as the work now under consideration must be to all practical astronomers, its real utility is at present very much circumscribed by its extreme scarcity. It is believed there are very few copies in this kingdom, and the book is only to be obtained by direct importation from Palermo *. This evil admits of a remedy, and I sincerely hope its application will not long be delayed.

8. "Fundamenta Astronomiæ, pro anno 1755, deducta ex observationibus viri incomparabilis James Bradley in speculâ astronomicâ Grenovicensi, per annos 1750–1762 institutis, auctore Friderico Wilhelmo Bessel." Fol. Regiomonti, 1818.

The observations of our illustrious countryman Bradley, on the fixed stars, remained upwards of 30 years in manuscript, to the great injury of science †. They were at length printed by the University of Oxford, in two volumes folio, the first having appeared in 1798, and the second in 1805. It was reserved for the German astronomer Bessel, to draw from these materials the important results they were capable of affording. After upwards of six years' application, he has produced a work which reflects the highest credit both on his talents and his industry. An analysis of the contents was given in the Phil. Mag. previously to its publication. I shall here notice only such parts as more particularly relate to my present subject.

Section 10 contains the catalogues of stars; the following passages are extracted from the introduction to that section.

"Prior catalogorum, quos hic trado astronomis, continet omnes à Bradleio observatas stellas, quas verè in cœlo adesse intellexi, quarum copia est 3222; alteri insunt observationes, quæ ad annum 1755 non poterant reduci. Inveniuntur autem prioris catalogi stellæ, vel in magno Piazzii catalogo, vel in Lalandii Historiâ Celesti, vel in catalogo Flamsteediano, vel in Indice ad Bodii Uranographiam pertinente: aliæ etiam à meipso quærebantur atque observabantur in speculâ Regiomontanâ: ceteræ denique à Bradleio pluries variisque temporibus animadversæ sunt. Ubi stellas è Piazzii libro recepi, loci determinationem pro anno 1755 comparavi cum ejusdem auctoris determinatione, ad annum 1800 spectante, qualem in novâ catalogi editione reperi: præterea nunquam non indicavi, quo loco quærendæ sint stellæ notatæ. Denominationes diligenter examinavi, neque ulli incertitudini hac

* I cannot omit noticing, that the last edition is printed with English types, and, I suspect, also on English paper.

† A catalogue of 387 stars founded on a small portion of Bradley's observations, was printed in the Nautical Almanac for 1773, and is the same that is usually referred to as "Bradley's Catalogue." According to Lalande, there are no fewer than 50 errors in it.

in re obnoxium esse catalogum, præstare audeo. Quinque vulgò computatæ sunt observationes cujusvis stellæ; ubicunque enim in commentariis Bradleianis quinque tantummodò paucioresve scriptæ extabant, nullam earum omisi. Contrà, ubi plures observationes aderant, sæpius eum egressus sum numerum. Hoc modo effectum esse puto ut catalogo optabilis sit subtilitas.

"Comparationi, cum magno Piazzi catalogo institutæ, subtilissimum inesse puto proprii motûs stellarum fixarum determinationem, quam pro hodiernâ rerum conditione expectare quis possit. Annuæ adscensionum rectarum ac declinationum mutationes computabantur pro annis 1755 et 1800: neque minutæ secundæ partibus centenariis, ut vulgò fit, contentus fui, sed descendi ad partes milliarias, ut in comparatione amborum catalogorum certitudinem assequerer."

The proper motion of the stars forms the subject of the 12th section, wherein Professor Bessel has obtained the following results: that out of 2959 stars employed in his comparison, there are 1375 whose annual proper motion amounts to 0·″1 of a great circle; 425 which amount to 0·″2; and only 71 which extend to 0·″5.

From the preceding account of the existing Catalogues, the reader will form his own opinion. The writer of this, however, believes that most persons will consider the assertion made at the commencement of the article, sufficiently established.

LXVI. *Reply to Mr.* Ivory's *Remarks on the Series of the Article* Cohesion. *In a Letter to the Editor.*

Sir, — I am perfectly ready to admit and to lament the inconveniences of the series in the article Cohesion of the *Encyclopædia Britannica;* but I must beg to be allowed to assert and to lament *still more,* that both Mr. Ivory's methods of computation are *still more imperfect;* and that they are proved to be so, by the very arguments which he has adduced *against the series* in question.

In fact, all these arguments tend to show, that the series, taken for any *limited* number of terms, must give the depression *too great,* and may sometimes make it *much too great;* since Mr. Ivory does not attempt to deny, that all its terms must be positive; and that, the more terms we compute, the smaller will the depression come out.

If, indeed, Mr. Ivory's computation had made the depression ten times as remote from that which was assigned by his predecessor as he has done, it would have been very difficult to have proved its error by the same mode of reasoning, provided that he had

had made the depression *less* instead of *greater*: but he has not attempted in the slightest degree to invalidate the chain of arguments, by which it is undeniably demonstrated, in the Journal of the Royal Institution, that the depression *must be less* than ·00418, for a tube of six-tenths of an inch in diameter, and that it *cannot* therefore be either ·00443 or ·00431.

While this simple computation remains uncorrected, it must be allowed that Mr. Ivory's ingenious refinements have only furnished him with *two methods*, both of which are in error to the extent of *ten or twelve* times the supposed unit, if not, like his table in the Supplement, to the extent of *twenty*. It may however still be hoped, that his favourite "exponentials" may afford him a third formula, which will again enable him to *bisect* this error: and that he may have resolution to proceed in inventing an *infinite series of series*, approaching by degrees to the accuracy of that, which was *first* applied to the solution of the problem by the *original author* of the whole investigation, in its present form.

My object in these remarks is *truth*, not *victory*; for I do not pretend to be a competitor for victory with Mr. Ivory in point of analytical ingenuity. But what I do see clearly I will assert boldly; and will not give way to high authority, against that which appears to me to be plain matter of fact.

I am, sir,

Your very obedient servant,

London, 8th May 1821. S. B. L.

POSTSCRIPT.—Mr. Ivory observes, that he has not been able to accomplish what he had in view by the employment of the Taylorian theorem; although he alludes to that part of the *Elementary Illustrations of the Celestial Mechanics*, in which the way of applying the theorem to the problem in question is particularly pointed out. It appears, therefore, that he was disposed to object, for some unassigned reason, to the accuracy of that analysis: for it is impossible to conceive that he could have felt any difficulty in following its steps, if he had approved it: and it becomes necessary to show that the virtues of the "universal solvent" have not, in this case at least, been exaggerated.

For a tube six-tenths of an inch in diameter, the convergence of the original series is amply sufficient, as far as $\frac{1}{4}$ of an inch from the axis, the high powers of $\frac{1}{4}$ being considerably smaller than those of $\frac{3}{10}$. We readily obtain from the Supplement, p. 220, when $x = \frac{1}{4}$, the series $s = 3{\cdot}6251\ bx + 3{\cdot}2112\ b^3x^3 + 13{\cdot}9\ b^5x^5$, b being here supposed ·4160, and $bx = {\cdot}1040$, whence $s = {\cdot}37701 + {\cdot}00364 + {\cdot}00019 + [{\cdot}00002] = {\cdot}38086$; the inclination being 22° 23′, the cosine ·92463, and the tan-

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gent ·41201. We shall also have, for the ordinate, $y = \frac{2b}{q} +$ $1{\cdot}05636\, bx^2 + 2{\cdot}807\ b^3x^4 + 19\, b^5x^6 = {\cdot}004160 + {\cdot}027465$ $+ {\cdot}001071 + {\cdot}000057 + [{\cdot}000003] = {\cdot}032756$, with very little uncertainty, even with regard to the last place of decimals.

We may now proceed to compute the remaining increment of the sine, by the method laid down in the *Elementary Illustrations*, p. 95. The original equation being here $\int yx\mathrm{d}x = msx$, or $q\int yx\mathrm{d}x = sx$, we have $qyx\mathrm{d}x = s\mathrm{d}x + x\mathrm{d}s$, $qy = \frac{s}{x} + \frac{\mathrm{d}s}{\mathrm{d}x}$, and $\frac{\mathrm{d}s}{\mathrm{d}x} = qy - \frac{s}{x} = \mathrm{A}$, for the first coefficient of Δx; then $\mathrm{d}\frac{\mathrm{d}s}{\mathrm{d}x} = q\mathrm{d}y - \frac{\mathrm{d}s}{x} + \frac{s\mathrm{d}x}{xx}$; and, if t be the tangent of the inclination, $\frac{\mathrm{d}y}{\mathrm{d}x}$ being equal to t, $\frac{\mathrm{d}^2s}{\mathrm{d}x^2} = qt - \frac{\mathrm{d}s}{x\mathrm{d}x} + \frac{s}{xx} =$ $qt - \frac{\mathrm{A}}{x} + \frac{s}{xx} = \mathrm{B} = qt - \frac{qy}{x} + \frac{s}{xx} + \frac{s}{xx} = q\left(t - \frac{y}{x}\right) + \frac{2s}{xx}$. Again, since $\mathrm{d}t = \frac{\mathrm{d}s}{u^3}$, if u be the cosine of the inclination, $\mathrm{d}\frac{\mathrm{d}^2s}{\mathrm{d}x^2} = \frac{q\mathrm{d}s}{u^3} - \frac{\mathrm{dA}}{x} + \frac{\mathrm{A}\mathrm{d}x}{xx} + \frac{\mathrm{d}s}{xx} - \frac{2s\mathrm{d}x}{x^3}$, and $\frac{\mathrm{d}^3s}{\mathrm{d}x^3} = \mathrm{C} = \frac{q\mathrm{A}}{u^3}$ $- \frac{\mathrm{B}}{x} + \frac{\mathrm{A}}{xx} + \frac{\mathrm{A}}{xx} - \frac{2s}{x^3} = \mathrm{A}\left(\frac{q}{u^3} + \frac{2}{xx}\right) - \frac{\mathrm{B}}{x} - \frac{2s}{x^3}$. Lastly for D, since $\mathrm{d}u = -t\mathrm{d}s$, and $\frac{\mathrm{d}u}{\mathrm{d}x} = -\frac{t\mathrm{d}s}{\mathrm{d}x} = -t\mathrm{A}$, we obtain $\mathrm{d}\frac{\mathrm{d}^3s}{\mathrm{d}x^3} = \mathrm{dA}\left(\frac{q}{u^3} + \frac{2}{xx}\right) - \mathrm{A}\left(\frac{3q\mathrm{d}u}{u^4} + \frac{4\mathrm{d}x}{x^3}\right) - \frac{\mathrm{dB}}{x} + \frac{\mathrm{B}\mathrm{d}x}{xx} - \frac{2\mathrm{d}s}{x^3}$ $+ \frac{6s\mathrm{d}x}{x^4}$, and $\frac{\mathrm{d}^4s}{\mathrm{d}x^4} = \mathrm{B}\left(\frac{q}{u^3} + \frac{2}{xx}\right) + \mathrm{A}\left(\frac{3qt\mathrm{A}}{u^4} - \frac{4}{x^3}\right) - \frac{\mathrm{C}}{x} + \frac{\mathrm{B}}{xx}$ $- \frac{2\mathrm{A}}{x^3} + \frac{6s}{x^4} = \frac{3\mathrm{A}^2qt}{u^4} - \frac{6\mathrm{A}}{x^3} + \mathrm{B}\left(\frac{q}{u^3} + \frac{3}{xx}\right) - \frac{\mathrm{C}}{x} + \frac{6s}{x^4}$. We thus obtain the series $\Delta s = {\cdot}25139 + {\cdot}08548 + {\cdot}02314 +$ ${\cdot}00655 + [{\cdot}00218]$, making the remainder $\frac{1}{3}$ of the last term, since the ratio of the preceding terms differs little from $\frac{1}{4}$; so that we have $s = {\cdot}74960$; which is less than ·75 by ·0004, or about $\frac{1}{2000}$ of the whole only, without a *possibility* of any error in the *last* place of the depression ·00416, if the numerical computation is correct; unless indeed the defect of the series should still have made even this value a *little too great*.

LXVII. *No-*

LXVII. *Notices respecting New Books.*

Journal of a Voyage for the Discovery of a North West Passage from the Atlantic to the Pacific; performed in the Years 1819–20, in His Majesty's Ships Hecla and Griper, under the Orders of William Edward Parry, R.N., F.R.S. and Commander of the Expedition. With an Appendix, containing the Scientific and other Observations. Published by Authority of the Lords Commissioners of the Admiralty. 4to. pp. 489. London, 1821. 3*l.* 13*s.* 6*d.*

Every person must recollect the anxiety—hardly mixed at last even with a shade of hope—that was entertained for the fate of this Expedition, when the season had once arrived that precluded the possibility of a return without first passing a winter in the Arctic region. The vessels however, as our readers know, returned at length in safety, and in the volume before us we have the record of their enterprise—a narrative which, considering the monotonous scene in which the gallant men engaged in it were obliged to put forth their energies, is indeed highly interesting. Our limits necessarily compel us to be brief in our notice of this work, so valuable to navigation and geographical inquiry.

Capt. Parry has prefixed to his Journal a copy of his Instructions, which appear to have been drawn up with considerable judgement. They were in substance, that he should make the best of his way to Davis's Strait, and, when the ice was sufficiently open to admit his approach to the western shores of the strait, that he should advance to the northward, as far as the opening into Sir James Lancaster's Sound; explore the bottom of that sound,—pass through it, if possible, and get to Behring's Straits. Should he fail in making a passage through this sound, he was to examine Alderman Jones's Sound, and if he could not pass through it, then to try Sir Thomas Smith's Sound, in every part of it. Should he fail here also, he was to return to the southward, down Baffin's Bay, and endeavour to make way through Cumberland's Strait, or any opening that might lead him to the seas adjoining the eastern or northern coast of America, and pursue his voyage along that coast, to the northward or westward, to Behring's Straits. Although this was the order in which the various attempts were recommended by the Admiralty to be made, yet Captain Parry had the discretionary power to make them in such order as appeared to him most advantageous. We may observe, *en passant*, that Captain Parry, following the orders of the Admiralty, first attempted to pass through Lancaster Sound, and succeeded. If he had accomplished his passage through Behring's Strait, he was then to proceed to Kamtschatka, and send from

 thence,

thence, through the Russian governor, a duplicate of his journals to London. From Kamtschatka he was to proceed to the Sandwich Islands or Canton, or any other place he might think proper, to refit the ships and refresh the crews, and then to return to England by such route as he might deem most convenient. Captain Parry was also allowed to winter in the Arctic regions, if he deemed it necessary. He was directed to make such observations as might tend to the improvement of astronomy, geography, and navigation; and to collect and preserve such specimens of the animal, mineral and vegetable kingdoms as he might meet with. The Lords of the Admiralty, relying with a just and liberal confidence on the well-known zeal and talents of Captain Parry, left much to his discretion and judgement.

The Hecla was commanded by Captain (then Lieut.) Parry; the Griper by Lieut. Liddon. Both had been prepared with great attention for the voyage, and supplied with every requisite that could be anticipated as likely to prove useful. The number of persons on board both was ninety-four; the greater part of the seamen had been employed in Captain Ross's voyage; and every individual was allowed double the ordinary pay of His Majesty's Navy.

The two vessels sailed from the Nore on the 10th of May 1819; and on the 18th of June, when about lat. 58° 52′ and long. 48° 12′, they fell in with the first 'stream' of ice, and soon afterwards they saw several icebergs. On the evening of the 24th, some curious effects of atmospheric refraction were observed, the low ice being at times considerably raised in the horizon, and constantly altering its appearance. The next day, the ice through which they had been towing, closed together so rapidly, that the crews had scarcely time to hoist up the boats before the ships were immoveably beset. 'It is impossible,' says Captain Parry, 'to conceive a more helpless situation than that of a ship thus beset, when all the power that can be applied will not alter the direction of her head a single degree of the compass.' Some of the gentlemen walked a mile or two from the ships, and imagined they saw marks of a sledge on the ice; but in this, Captain Parry thinks, they were mistaken. The ships remained locked in the ice until the 30th, when they were able to move them a little. 'A southerly swell dashing the loose ice with tremendous force against the bergs, sometimes raised a white spray over the latter, to the height of more than one hundred feet, and being accompanied with a loud noise, exactly resembling the roar of distant thunder, presented a scene at once sublime and terrific.' On the 4th of July, the Hecla attempting to push through the ice, was for some time at the mercy of a swell of the ocean, which drifted her fast towards the bergs; but she was, fortunately, brought

back

back into clear water. They were now near the middle of the narrowest part of Davis's Straits, and had the opportunity of confirming the accuracy of that celebrated and able navigator. Streams of the purest water were often found flowing from the icebergs; and from this time to the end of the voyage, snow-water was exclusively made use of on board the ships for every purpose. During the summer months it was found in abundance in pools, upon the floes and icebergs; and in the winter, snow was dissolved in the copper for their daily consumption. On the 20th of July, the ships crossed a stream of ice, of which the breadth scarcely exceeded three hundred yards, and which occupied them constantly for five hours. The next day they drifted towards an iceberg, which was one hundred and forty feet high, and which, from the soundings made near it, must have been a-ground in one hundred and twenty fathoms, so that its whole height was about eight hundred and sixty feet. Of this iceberg Captain Parry gives a view, which is awfully grand, from a sketch by Lieutenant Beechey. In the course of the voyage they frequently had to saw through masses of ice; but they sometimes ran through 'bay-floes, which were from four to six inches thick, ploughing up the ice before the ship's stem, at the rate of five miles an hour.

'If they were not very broad, the Hecla did not lose her way in passing through them. Frequently, however, she was stopped in the middle, which made it necessary to saw and break the ice a-head, till she made another start, and, having run a short distance in clear water, was again imbedded in the same manner. We (says the author) passed one field of ice about ten feet in thickness and many miles in length, as we could not see over it from the mast head.'

On the 28th of July, the ships had passed every impediment which obstructed their passage into Sir James Lancaster's Sound. The breadth of the barrier of ice, which occupies the middle of Baffin's Bay, and which had never before been crossed in this latitude at the same season, was eighty miles in a N. 63° W. direction. Captain Parry expresses it as his opinion, that, by taking advantage of every little opening that is afforded, a strong built vessel, of proper size and weight, may, in most seasons, be pushed through this barrier. Sir James Lancaster's Sound was now open to the westward, and the two best months in the year, for the navigation of these seas, were yet to come.

On the 1st of August Captain Parry entered Lancaster Sound, which has obtained much celebrity from the very opposite opinions which have been held with regard to it. To him it was particularly interesting, as being the point to which his instructions more particularly directed his attention. On the 2d, they

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sounded with the deep sea clamms, and found 1050 fathoms by the line; but as, where the soundings exceed five or six hundred fathoms, there is some uncertainty, Captain Parry supposes the actual depth to have been from eight to nine hundred fathoms. Sir George Hope's monument, which had been thought an island in the former voyage, was now discovered to be a dark-looking and conspicuous hill on the main land. On the 30th, the Hecla had gained somewhat on the Griper, and was in lat. 74° 25′ 31″, long. 80° 04′ 30″. Of the enthusiasm which now animated the crew, Captain Parry thus speaks:

'Being favoured, at length, by the easterly breeze which was bringing up the Griper, and for which we had long been looking with much patience, a crowd of sail was set, to carry us with all rapidity to the westward. It is more easy to imagine than describe the almost breathless anxiety which was now visible in every countenance, while, as the breeze increased to a fresh gale, we ran quickly up the sound. The mast-heads were crowded by the officers and men during the whole afternoon; and an unconcerned observer, if any could have been unconcerned on such an occasion, would have been amused by the eagerness with which the various reports from the crow's nest were received; all, however, hitherto favourable to our most sanguine hopes.'

On the following day, they came near two inlets, in lat. 74° 15′ 53″ N. long. 86° 30′ 30″; these they named Burnet's Inlet and Stratton Inlet. The cliffs on this part of the coast present a singular appearance, being stratified horizontally, and having a number of regular projecting masses of rock, broad at the bottom, and coming to a point at the top, resembling so many buttresses raised by art at equal intervals. Some islands, to which the name of Prince Leopold was given, were also stratified horizontally, but without the buttress-like projections.

From the time that Capt. Parry first entered Lancaster's Sound, the sluggishness of the compasses, as well as the amount of their irregularity, had been found to increase rapidly though uniformly. The irregularity became more and more obvious as they advanced to the southward. By observation they found, that when the true course of the Hecla was about S.S.W., the binnacle and azimuth compasses at the same time agreed in showing N.N.W. ½ W., making the variation to be allowed on that course, eleven points and a half westerly. It was evident, therefore, that a very material change had taken place in the dip or the variation, or in both these phænomena, which rendered it probable that they were making a very near approach to the magnetic pole.

'We now, therefore,' says Captain Parry, 'witnessed, for the first time, the curious phænomenon of the directive power of the needle becoming so weak as to be completely overcome by the attraction

attraction of the ship; so that the needle might now be properly said to point to the north pole of the ship. It was only, however, in those compasses in which the lightness of the cards, and great delicacy in the suspension, had been particularly attended to, that even this degree of uniformity prevailed; for, in the heavier cards, the friction upon the points of suspension was much too great to be overcome even by the ship's attraction, and they consequently remained indifferently in any position in which they happened to be placed. For the purposes of navigation, therefore, the compasses were from this time no longer consulted; and in a few days afterwards the binnacles were removed, as useless lumber, from the deck to the carpenter's store-room, where they remained during the rest of the season, the azimuth compass alone being kept on deck, for the purpose of watching any changes which might take place in the directive power of the needle: and the true courses and direction of the wind were in future noted in the log-book, as obtained to the nearest quarter-point, when the sun was visible, by the azimuth of that object and the apparent time.'

On the following day (the 8th of August) the directive power of the magnet seemed to be weaker than ever; for the north-pole of the needle, in Captain Kater's steering compass, in which the friction is almost entirely removed by a thread suspension, was observed to point steadily towards the ship's head, in whatever direction the latter was placed. An accidental circumstance convinced Captain Parry that there was no current setting constantly in one direction. A small piece of wood was picked up, which appeared to have been the end of a boat's yard, and which caused sundry amusing speculations among the gentlemen on board, who felt rather mortified to think that a ship had been there before them, and that, therefore, they were not entitled to the honour of the first discovery. A stop was suddenly put to this and other ingenious inductions, by the information of one of the seamen that he had dropped it out of his boat a fortnight before.

The vessels continued their progress, and several bays, capes, and headlands were discovered, and received names by the voyagers. On the 22nd they had a clear and extensive view to the northward, free from ice; and they now felt that they had actually entered the Polar Sea. The magnificent opening, through which their passage had been effected, from Baffin's Bay to a channel dignified with the name of Wellington, was called Barrow's Straits, after the Secretary of the Admiralty.

In latitude 75° 03′ 12″, long. 103° 44′ 37″, an island was discovered, and Captain Sabine, with two other officers, landed on it near the east point, which was called Cape Gillman. The gentlemen reported, on their return, that

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'The remains of Esquimaux habitations were found in four different places. Six of these, which Captain Sabine had an opportunity of examining, and which are situated on a level sandy bank, at the side of a small ravine near the sea, are described by him as consisting of stones rudely placed in a circular or rather elliptical form. They were from seven to ten feet in diameter; the broad flat sides of the stones standing vertically, and the whole structure, if such it may be called, being exactly similar to that of the summer huts of the Esquimaux, which had been seen at Hare Island the preceding year. Attached to each of them was a small circle, generally four or five feet in diameter, which had probably been the fire-place.'

The whole encampment appeared to have been deserted for several years; but very recent traces of the rein-deer and musk-ox were seen in many places. The steering of the vessels now became very difficult, and, says our author,

'The circumstances under which we were sailing, were, perhaps, such as never occurred since the early days of navigation. To the northward was the land; the ice, as we supposed, to the southward; the compasses useless; and the sun completely obscured by a fog, so thick that the Griper could only now and then be seen at a cable's length astern. We had literally, therefore, no mode of regulating our course, but by once more trusting to the steadiness of the wind; and it was not a little amusing, as well as novel, to see the quarter-master conning the ship by looking at the dog vane.'

On the 2nd of September a star was seen, being the first that had been visible for more than two months. Two days afterwards, namely, on the 4th, at a quarter past nine P.M., the ships crossed the meridian of 110° west from Greenwich, in the latitude of 74° 44′ 20″, by which they were entitled to the reward of 5000*l.* In order to commemorate the event, a bluff head-land that they had just passed was called Bounty Cape. On the following day they dropped anchor, for the first time since quitting the English coast, in a roadstead, which was called the Bay of the Hecla and Griper, and the crews landed on the largest of a groupe of islands, which was called Melville Island. 'The ensigns and pendants,' says Captain Parry, 'were hoisted as soon as we had anchored, and it created in us no ordinary feelings of pleasure to see the British flag waving, for the first time, in these regions, which had hitherto been considered beyond the limits of the habitable part of the world.'—They did not remain here many days, before parties ventured on shooting excursions, and three men, who had missed their way, were absent ninety-one hours, and exposed during three nights to the inclemency of the weather.

Captain Parry still attempted to gain a passage to the westward,

ward, and succeeded in getting along the coast of Melville Island to some distance; but there being no hope of penetrating further at that season, and the ice setting in very rapidly, he was induced to return to Hecla and Griper Bay, which he regained on the 24th of September. It was now necessary to cut a canal through the ice, and to draw the ships up it into the harbour. Two parallel lines were cut, distant from each other little more than the breadth of the large ships, and the ice then divided into rectangular pieces, which were again subdivided diagonally, and floated out of the canal. It was afterwards found necessary to sink the pieces of ice under the floe as they were cut. At three o'clock of the third day spent in these operations, the vessels reached their winter quarters, an event which was hailed with three hearty cheers by the united ships' crews. The group of islands that were discovered were called The North Georgian Islands.

The ships had now reached that station, where, in all probability, they were destined to remain for at least eight months, during three of which they were not to see the face of the sun. Every precaution was immediately taken for the security of the ships and the preservation of the various stores; the masts were dismantled, except the lower ones, and the planks of the housing erected, and afterwards roofed over with a cloth composed of wadding tilt. The crews of both vessels were in excellent health, which great care was still taken to preserve, by keeping the births and bed places as warm and dry as possible. The allowance of bread was reduced to two thirds; a pound of Donkin's preserved meat, together with one pint of vegetable or concentrated soup, per man, was substituted for a pound of salt beef weekly; a proportion of beer and wine was served instead of spirits; and a small quantity of sour krout and pickles, with as much vinegar as could be used, was issued at regular intervals. The daily proportion of lime juice and sugar, mixed with water, was drunk by each man, in presence of an officer appointed to attend to this duty. When any game was procured, it was served in lieu of the established allowance of meat; and in no one instance, either in quantity or quality, was the slightest preference given to the officers.

In regard to clothing, equal attention was paid to the comfort of every individual on board; and, now being in a state of leisure and inactivity, Capt. Parry projected the amusements of a theatre, of which Lieut. Beechey was stage manager; and a weekly newspaper, to be called "The North Georgia Gazette and Winter Chronicle," of which Capt. Sabine undertook to be the editor. These had the happy effect of diverting the mind from the gloomy prospect which would sometimes obtrude itself on the stoutest heart.

Some deer having been seen near the ships on the 10th of October, a party was despatched after them, and, being led on by the ardour of pursuit, forgot Capt. Parry's order that every person should be on board before sunset.

'John Pearson, a marine belonging to the Griper, who was the last that returned on board, had his hands severely frost-bitten, having imprudently gone away without mittens, and with a musket in his hand. A party of our people most providentially found him, although the night was very dark, just as he had fallen down a steep bank of snow, and was beginning to feel that degree of torpor and drowsiness which, if indulged, inevitably proves fatal. When he was brought on board, his fingers were quite stiff, and bent into the shape of that part of the musket he had been carrying; and the frost had so far destroyed the animation in his fingers on one hand, that it was necessary to amputate three of them a short time after, notwithstanding all the care and attention paid to him by the medical gentlemen. The effect which exposure to severe frost has, in benumbing the mental as well as the corporeal faculties, was very striking in this man, as well as in two of the young gentlemen who returned after dark, and of whom we were anxious to make inquiries respecting Pearson. When I sent for them into my cabin, they looked wild, spoke thick and indistinctly, and it was impossible to draw from them a rational answer to any of our questions. After being on board for a short time, the mental faculties appeared gradually to return with the returning circulation, and it was not till then that a looker-on could easily persuade himself that they had not been drinking too freely. To those who have been much accustomed to cold countries, this will be no new remark; but I cannot help thinking (and it is with this view that I speak of it) that many a man may have been punished for intoxication, who was only suffering from the benumbing effects of frost; for I have more than once seen our people in a state so exactly resembling that of the most stupid intoxication, that I should certainly have charged them with that offence, had I not been quite sure that no possible means were afforded them on Melville Island, to procure any thing stronger than snow-water.'

The 4th of November was the last day that the sun was seen above the horizon, but the weather was not sufficiently clear to allow the scientific gentlemen to make any observations on the disappearance of that cheering orb, 'of this great world both eye and soul.' The next day the theatre was opened, and *Miss in her Teens* performed; a new Christmas piece was also produced, which was received with great *eclat* by the audience. The circumstances under which the crews were situated being such as never before occurred, it cannot be uninteresting to know in

in what manner they passed their time, during three months of nearly total darkness, in the middle of a severe winter, and in a climate where Europeans never wintered before:

'The officers and quarter-masters were divided into four watches, which were regularly kept, as at sea, while the remainder of the ship's company were allowed to enjoy their night's rest undisturbed. The hands were turned up at a quarter before six, and both decks were well rubbed with stones and warm sand before eight o'clock, at which time, as usual at sea, both officers and men went to breakfast. Three-quarters of an hour being allowed after breakfast for the men to prepare themselves for muster, we then beat to divisions punctually at a quarter past nine, when every person on board attended on the quarter-deck, and a strict inspection of the men took place, as to their personal cleanliness, and the good condition, as well as sufficient warmth, of their clothing. The reports of the officers having been made to me, the people were then allowed to walk about, or, more usually, to run round the upper deck, while I went down to examine the state of that below, accompanied, as I before mentioned, by Lieut. Beechey and Mr. Edwards. The state of this deck may be said, indeed, to have constituted the chief source of our anxiety, and to have occupied by far the greatest share of our attention at this period. Whenever any dampness appeared, or, what more frequently happened, any accumulation of ice had taken place during the preceding night, the necessary means were immediately adopted for removing it; in the former case, usually by rubbing the wood with cloths, and then directing the warm air-pipe towards the place; and in the latter, by scraping off the ice, so as to prevent its wetting the deck by any accidental increase of temperature. In this respect, the bed-places were particularly troublesome; the inner partition, or that next the ship's side, being almost invariably covered with more or less dampness or ice, according to the temperature of the deck during the preceding night. This inconvenience might, to a great degree, have been avoided, by a sufficient quantity of fuel to keep up two good fires on the lower dock, throughout the twenty-four hours; but our stock of coals would by no means permit this, bearing in mind the possibility of our spending a second winter within the Arctic circle; and this comfort could only, therefore, be allowed on a few occasions, during the most severe part of the winter.

'In the course of my examination of the lower deck, I had always an opportunity of seeing those few men who were on the sick list, and of receiving from Mr. Edwards a report of their respective cases; as also of consulting that gentleman as to the means of improving the warmth, ventilation, and general comfort of the inhabited parts of the ship. Having performed this duty,

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we returned to the upper deck, where I personally inspected the men; after which, they were sent out to walk on shore when the weather would permit, till noon, when they returned on board to their dinner. When the day was too inclement for them to take this exercise, they were ordered to run round and round the deck, keeping step to a tune on the organ, or, not unfrequently, to a song of their own singing. Among the men were a few who did not at first quite like this systematic mode of taking exercise; but when they found that no plea, except that of illness, was admitted as an excuse, they not only willingly and cheerfully complied, but made it the occasion of much humour and frolic among themselves.

'The officers, who dined at two o'clock, were also in the habit of occupying one or two hours in the middle of the day in rambling on shore, even in our darkest period, except when a fresh wind and a heavy snow drift confined them within the housing of the ships. It may be well imagined that at this period there was but little to be met with in our walks on shore, which could either amuse or interest us. The necessity of not exceeding the limited distance of one or two miles, lest a snow-drift, which often rises very suddenly, should prevent our return, added considerably to the dull and tedious monotony which, day after day, presented itself. To the southward was the sea, covered with one unbroken surface of ice, uniform in its dazzling whiteness, except that, in some parts, a few hummocks were seen thrown up somewhat above the general level. Nor did the land offer much greater variety, being almost entirely covered with snow, except here and there a brown patch of bare ground in some exposed situations, where the wind had not allowed the snow to remain. When viewed from the summit of the neighbouring hills, on one of those calm clear days, which not unfrequently occurred during the winter, the scene was such as to induce contemplations, which had, perhaps, more of melancholy than of any other feeling. Not an object was to be seen on which the eye could long rest with pleasure, unless when directed to the spot where the ships lay, and where our little colony was planted. The smoke which there issued from the several fires, affording a certain indication of the presence of man, gave a partial cheerfulness to this part of the prospect; and the sound of voices, which during the cold weather could be heard at a much greater distance than usual, served now and then to break the silence which reigned around us,—a silence far different from that peaceable composure which characterizes the landscape of a cultivated country; it was the death-like stillness of the most dreary desolation, and the total absence of animated existence. Such, indeed, was the want of objects to afford relief to the eye, or amusement to the mind,

mind, that a stone of more than usual size appearing above the snow, in the direction in which we were going, immediately became a mark on which our eyes were unconsciously fixed, and towards which we mechanically advanced.'

On the probable existence and accomplishment of a north-west passage into the Pacific Ocean, Captain Parry expresses himself thus:

'Of the existence of such a passage, and that the outlet will be found in Behring's Strait, it is scarcely possible, on an inspection of the map, with the addition of our late discoveries, and in conjunction with those of Cook and Mackenzie, any longer to entertain a reasonable doubt. In discovering one outlet from Baffin's Bay into the Polar Sea, and finding that sea studded with numerous islands, another link has at least been added to the chain of evidence upon which geographers have long ventured to delineate the northern coast of America, by a dotted line from Icy Cape westward, to the rivers of Mackenzie and Hearne, and thence to the known part of the coast to the north of Hudson's Bay, in the neighbourhood of Wager River; while, at the same time, considerable progress has been made towards the actual accomplishment of the desired passage, which has for nearly three centuries engaged the attention of the maritime nations of Europe.

'The success which attended our efforts during the season of 1819, after passing through Sir James Lancaster's Sound, was such as to inspire even the least sanguine among us with reasonable hope of the complete accomplishment of our enterprise, before the close of the next season. In entertaining such a hope, however, we had not rightly calculated on the severity of the climate with which we had to contend, and on the consequent shortness of the season (not exceeding seven weeks) in which it is possible to perform the navigation of that part of the Polar Sea. Although it must be admitted, that there is something peculiar about the south-west end of Melville Island, extremely unfavourable to navigation; yet it is also certain, that the obstructions we met with from ice, both as to its thickness and extent, were found generally to increase as we proceeded westward after passing through Barrow's Strait. That we should find this to be the case, might perhaps have been reasonably anticipated, because the proximity to a permanently open sea appears to be the circumstance which, of all others, tends the most to temper the severity of the Polar regions, in any given parallel of latitude. On this account, I should always expect to meet with the most serious impediments about mid-way, between the Atlantic and Pacific Oceans; and having once passed that barrier, I should

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as confidently hope to find the difficulties lessen in proportion as we advanced towards the latter sea; especially as it is well known, that the climate of any given parallel on that side of America is, no matter from what cause, very many degrees more temperate than on the eastern coast.

'But, although it is evident that climate does not wholly depend on latitude, but on other circumstances also (principally, perhaps, those of locality above mentioned), yet it can scarcely be doubted that, on any meridian to the north of America, for instance, 114° west, where we were stopped, the general climate would be found somewhat better, and the navigable season longer, in the latitude of 69° than in that of 75°, near which we wintered. For this reason, it would perhaps be desirable, that ships endeavouring to reach the Pacific by this route, should keep, if possible, on the coast of America; and the lower in latitude that coast may be found, the more favourable will it prove for this purpose.

'Our experience, I think, has clearly shown that the navigation of the Polar Seas can never be performed with any degree of certainty, without a continuity of land. It was only by watching the occasional openings between the ice and the shore, that our late progress to the westward was effected; and had the land continued in the desired direction, there can be no question that we should have continued to advance, however slowly, towards the completion of our enterprise. In this respect, therefore, as well as in the improvement to be expected in the climate, there would be a manifest advantage in making the attempt on the coast of America, where we are sure that the land will not fail us. The probability of obtaining occasional supplies of wood, game, and anti-scorbutic plants; the chance of being enabled to send information by means of the natives; and the comparative facility with which the lives of the people might be saved, in case of serious and irreparable accidents happening to the ships, are also important considerations, which naturally serve to recommend this route. Should the sea on the coast of America be found moderately deep, and shelving towards the shore (which, from the geological character of the known parts of the continent to the south, and of the Georgian Islands to the north, there is reason to believe would be the case for a considerable distance to the westward), the facility of navigation would be much increased on account of the grounding of the heavy masses of ice in water sufficiently deep to allow the ships to take shelter behind them, at such times as the floes close in upon the land. Further to the westward, where the primitive formation, and perhaps even a continuation of the Rocky Mountains, is to be expected, a steep and

and precipitous shore would probably occur, a circumstance which the foregoing narrative has shown to be attended with much comparative uncertainty and risk.

'The question which naturally arises, in the next place, relates to the most likely means of getting to the coast of America, so as to sail along its shores. It would, in this respect, be desirable to find an outlet from the Altantic into the Polar Sea, as nearly as possible in the parallel of latitude in which the northern coast of America may be supposed to lie: as, however, we do not know of any such outlet from Baffin's Bay, about the parallels of 69° to 70°, the attempt is, perhaps, to be made with better chance of success in a still lower latitude, especially as there is a considerable portion of coast that may reasonably be supposed to offer the desired communication, which yet remains unexplored. Cumberland Strait, the passage called Sir Thomas Rowe's Welcome, lying between Southampton Island and the coast of America, and Repulse Bay, appear to be the points most worthy of attention; and, considering the state of uncertainty in which the attempts of former navigators have left us, with regard to the extent and communication of these openings, one cannot but entertain a reasonable hope, that one, or perhaps each of them, may afford a practicable passage into the Polar Sea.

'So little, indeed, is known of the whole of the northern shore of Hudson's Strait, which appears, from the best information, to consist chiefly of islands, that the geography of that part of the world may be considered altogether undetermined; so that an expedition, which should be sent to examine those parts, would soon arrive upon ground never before visited, and in which, from an inspection of the map in its present state, there certainly does seem more than an equal chance of finding the desired passage. It must be admitted, however, that any notions we may form upon this question, amount after all to no more than conjecture. As far as regards the discovery of another outlet into the Polar Sea, to the southward of Sir James Lancaster's Sound, it is evident that the enterprise is to be begun again; and we should be cautious, therefore, in entertaining too sanguine a hope of finding such a passage, the existence of which is still nearly as uncertain as it was two hundred years ago, and which possibly may not exist at all.

'In the course of the foregoing narrative, it may have been remarked, that the westerly and north-westerly winds were always found to produce the effect of clearing the southern shores of the North Georgian islands of ice, while they always brought with them clear weather, which is essentially necessary in prosecuting discoveries in such a navigation. This circumstance, together with the fact of our having sailed back in six days from the

the meridian of Winter Harbour to the entrance of Sir James Lancaster's Sound, a distance which it required five weeks to traverse when going in the opposite direction, seems to offer a reasonable ground for concluding, that an attempt to effect the north-west passage might be made with a better chance of success, from Behring's Strait, than from this side of America. There are some circumstances, however, which, in my opinion, render this mode of proceeding altogether impracticable, at least for British ships. The principal of these arises from the length of the voyage which must first be performed, in order to arrive at the point where the work is to be begun. After such a voyage, admitting that no serious wear and tear have been experienced, the most important part of a ship's resources, namely, the provisions and fuel, must be very materially reduced, and this without the possibility of renewing them, to the extent necessary for such a service, and which can alone give confidence in the performance of an enterprise of which the nature is so precarious and uncertain.

'Nor should it be forgotten how injurious to the health of the crews, so sudden and extreme a change of climate would in all probability prove, as that which they must necessarily experience in going at once from the heat of the torrid zone into the intense cold of a long winter upon the northern shores of America. Upon the whole, therefore, I cannot but consider, that any expedition, equipped by Great Britain with this view, will act with greater advantage, by at once employing its best energies in the attempt to penetrate from the eastern coast of America, along its northern shore.'

The embellishments consist of maps, charts, and other engravings, to the number of twenty in all, and are, generally speaking, highly appropriate.

Recently published,

Printed uniformly with the foregoing,

The North Georgia Gazette and Winter Chronicle, a Newspaper, that was established on board the Ships employed in the Discovery of a North-West Passage, edited by Captain Edward Sabine, R.A. 10*s*. 6*d*.

The Young Navigator's Guide to the Sidereal and Planetary Parts of Nautical Astronomy; being the Theory and Practice of finding the Latitude and Longitude, and the Variation of the Compass, by the fixed Stars and Planets. To which is prefixed, the Description and Use of the New Celestial Planisphere. By Thomas Kerigan, Purser, R.N. Royal 18mo. 18*s*. bds.—The Planispheres sold separately at 5*s*. each.

Elementary Illustration of the Celestial Mechanics of Laplace. 8vo. 10*s*. 6*d*.

No. 48 of the *Mineral Conchology* of Great Britain, by Mr. James Sowerby, has appeared; and a *monthly* publication is now intended, in order to make up for the time unfortunately lost, since the less frequent publication was suspended in June last.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 5.—A PAPER was read "On the mean Density of the Earth," by Charles Hutton, LL.D.

Also, a paper "On the Separation of Iron from other Metals," by J. F. W. Herschel, Esq. To separate iron from the metals not precipitated by sulphuretted hydrogen, which it most usually contaminates (manganese, cerium, nickel, and cobalt), Mr. H. proposes to take advantage of a peculiarity in the peroxide of iron, in virtue of which it is incapable of subsisting in a neutral solution at the boiling temperature. If a solution of this peroxide be neutralized when cold, and then heated, a portion is deposited in the state of a subsalt, and the liquid becomes acid. If allowed to cool, and again neutralized, a fresh portion of the metallic contents separates on re applying the heat, and so on, till the quantity held in solution is no longer sensible to the most delicate reagents. If, on the other hand, the neutralization be performed *while actually boiling*, we attain this limit at one operation. Hence Mr. Herschel recommends the following process: Having peroxidized, by means of nitric acid, a solution containing iron and any of the above mentioned metals, drop into it, *while boiling*, carbonate of ammonia, till the acid reaction is entirely destroyed, and *even going a little beyond the point of exact neutralization* The whole of the iron to the last atom is separated, while the liquid retains in solution the other metallic oxides, *as well as the minute portion of their carbonates due to a trifling excess of the alkaline precipitant.* In the cases of cobalt and cerium, the alkaline carbonate may be added in considerable excess, without separating any of those metals; and their solution, so freed from iron, is then *a most delicate test* of the presence of the latter metal.

April 12.—A paper was read "On the Restoration of a Part of the Urethra in the Perinæum." By H. Earle, Esq. Communicated by the President Sir Humphry Davy.

May 3.—"Observations on the Variation of local Heat made amongst the Garrow Hills," by D. Scott, Esq., in a letter to W. T. Brande, Esq. Sec. R.S., were read. On the same evening a paper was read "On some subterraneous Trees discovered at the Foot of the Cliffs about a Mile to the Eastward of Mundsley." By Lieut. Jefferson Miles, R.N. In a letter to W. T. Brande, Esq, Sec. R.S. Also, "The Case of a diseased Enlargement of the Glands of the Neck." By John Howship, Esq.

ASTRONOMICAL SOCIETY OF LONDON.

May 11.—A Letter was read from the Rev. M. Ward, stating that on the evening of May 4th, he distinctly saw the spot, called Aristarchus, on the unenlightened portion of the moon's disc; having the appearance of a small comet. He presumes this has been mistaken, by many observers, for a volcano. Mr. Ward also gave an account of the occultation of the star 136 *Tauri*.

A paper was next read from Professor Moll, of Utrecht, giving an account of the late solar eclipse, as observed in many parts of Holland. It was annular at Amsterdam, contrary to the expectations of many persons. The formation and dissolution of the annulus presented that singular optical phænomenon, so frequently observed in annular eclipses: namely, an apparent adhesion of the periphery of the moon to that of the sun, as if formed of a glutinous substance. This remarkable occurrence had been pointed out by Mr. Baily, in his tract on this very eclipse, and he had thereby excited the attention of astronomers to observe it more particularly. It has been noticed by several persons in their accounts of the eclipse.

LXIX. *Intelligence and Miscellaneous Articles.*

MUSIC.

We understand that a very curious invention, or discovery, we know not well which to call it, has been made in the art of musical composition. As it has been described to us, cards are prepared, on each of which a bar of an air is arranged according to a certain rhythm and key. Four packs of these cards, marked A, B, C, and D, are mingled together; and as the cards are drawn, and arranged before a performer in the order of that series, it will be found that an original air is obtained. The cards hitherto made, we have been told, are as waltzes, and succeed perfectly. The invention may be called Musical Permutation. It has received, however, improperly, that of The Musical Kaleidoscope.

ASTRONOMICAL NOTICES.

The Editor has great pleasure in announcing that he hopes, should no unforeseen impediment occur, to be enabled, by the kindness of a friend, to commence publishing in a month or two, A New Catalogue of Fixed Stars. The present estimate comprises nearly 4000; but the catalogue, when completed, may a little exceed that number. It will be accompanied with notes, and prefaced by a short account of the materials employed in its compilation, comprising nearly the whole of the catalogues (both English and Foreign) which are held in estimation by astronomers. In order not to occupy an undue share of our pages, the portions of

of the catalogue (intended to be published in every alternate Number of our work) will contain from 300 to 400 stars each, according to circumstances, and will occupy each about nine or ten pages.

We believe but few astronomers will be found in this country, who are not fully satisfied that a publication of such tables is much wanted, since what we possess already of English production in this line sinks into insignificance, compared with the labours of foreign observers. The foreign publications of this kind are all very expensive; and what is still worse, the most important of them all, and that which will form the base of the New Catalogue, can hardly be procured at any price. It cannot be doubted that many persons in this country, who are fond of exploring the heavens, are prevented from turning their observations to any good purpose, for want of a sufficiently accurate and comprehensive catalogue of stars to assist their comparisons. The intended catalogue will furnish this desideratum.

A correspondent speaking of Mr. Utting's Tables, published in our recent Numbers, expresses himself thus:—" The Tables furnished by Mr. Utting relating to the Right Ascension and Declination of the Sun, I consider extremely valuable, as they are more accurate than any I know of in print. It is a pity, however, that he has assumed the secular diminution of obliquity, so much greater than is now known to be the true value. It would, perhaps, have been better to have given the Variations corresponding to a diminution of 60″, after the example of Mayer."

The Editor has also the satisfaction to announce that Mr. Utting has sent him some other curious and interesting Tables, relating to the Solar System, partly founded on the numbers given by M. Laplace, as revised in the 4th edition of his *Systême du Monde*, and which will be published in our succeeding Numbers.

To the Editor of the Philosophical Magazine.

Sir,—I cannot refrain from making a few remarks on the subject of Z. N.'s inquiry in your March Number, p. 234, respecting Daussy's Tables of Vesta. I admit the conciseness of the instructions is attended with considerable chance of error in applying them; yet I conceive that no difficulty can arise, which may not be removed by a little consideration, on the part of any one who is acquainted with the construction of such tables. The fact is, that the Editors have thought fit to print the Arguments to the Table of Equations with fewer places of figures in many instances, than in the Table of Epochs. This is unusual; and therefore, to prevent mistakes, they ought to have distinctly advertised the computer, how many figures (to the *right* hand) were to be struck off, in each particular case. Thus, among the perturbations

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in longitude, Args. 5, 6, 7, 8, 9, 10 and 29, are reduced from 4 places to 3: among those of the Rad Vector, Args. 1 and 3 are reduced from 5 places to 4; Args. 2 and 4, from 5 places to 3, and Args. 5, 6, 7, 8, 9, 10 and 29, from 4 places to 3.

The general direction given, to strike off one figure (that is on the *right hand*) from all the arguments, may do very well for calculating an ordinary ephemeris of the planet; where the Right Ascension is only wanted exact to the nearest minute of space, or to the nearest second of time: but it appears to me that it can be only partially resorted to in cases where every equation must be set down to the nearest tenth of a second, as is requisite where a comparison is to be made between observed and calculated positions. I am, &c.

April 3, 1821. *ΑΣΤΡΟΦΙΛΟΣ.*

BAROMETRIC OBSERVATIONS.

Epping, April 24, 1821.

Sir,—The following observations I have just received from Pocklington, and which were taken on the 12th of March and 9th of April, exactly at the times specified in the following table. Mr. Rogerson is a very attentive meteorologist, and it is to be hoped he will communicate, from time to time, to the Editor of the Phil. Mag., the result of his observations in this curious, interesting, and useful branch of philosophy. Yours truly,

Thomas Squire.

Hours.	Barom.	Thermo. att.	Thermo. det.	Wind.	Weather.
March 12th, 8h	29·713	50	41	S.W. by W.	Gentle breezes: broken dark clouds.
9	29·704	50	42	S.W.	Gentle breezes: clear, except some thin white clouds.
10	29·710	50	46	W.	Gentle breezes: broken dark clouds.
11	29·710	50	47	W. by S.	Gentle breezes: sky cloudy.
12	29 703	50	49	W. by S.	Gentle breezes: some white clouds.
April 9th, 8	29·878	57	55	W.	Gentle breezes: clear, except some flying gray clouds.
9	29·866	57	58	W.S.W.	Windy.—Do. and Do.
10	29·859	58	59	S.W.	Windy.—Do. and Do.
11	29·854	58	60	W.S.W.	Windy.—Do. and Do.
12	29·848	59	60	W. by S.	Strong breezes: sky somewhat vapoury, with flying gray clouds.

Crumpsall, Lancashire, May 17, 1821.

Sir,—The following observations were made on Monday the 14th instant; those under the head of Crumpsall, by myself, and those headed Manchester, by Mr. Thomas Hanson.

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CRUMPSALL.

1821. A.M.	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
May 14th 8h.	28.972	46·	41·	N.W. fresh.	Overcast with rain.
9	28.972	47	43·5	W. by N. do.	Cloudy with rain.
10	28.972	47	43·5	N.W. do.	Do. & faint sunshine
11	28.972	47·5	45	W.S.W. brisk.	Do. with sunshine.
12	28.972	48·	47 5	W.S.W. do.	Do.
1	28·970	49	49	W. by S. do.	Do.

MANCHESTER.

1821. A.M. May 14th	Bar.	Ther. att.	Ther. det.	Wind.	Weather.
8h.	29.220	54·5	45	N.W. by W. fresh.	Rainy.
9	29.220	55·5	49	N.W. by W. do.	Do.
10	29.220	55	48·5	N.W. by W. do.	Rainy with hail.
11	29.220	56	50	N.W. by W. do.	Do.
12	29.215	56·5	51	N.W. by W. do.	Clouds & sunshine.

If the height of the mercury in the barometer varied no more between the hours of eight and twelve on the morning of the 14th, in the different parts of the country where your correspondents reside, who contribute their monthly observations to your Magazine, than it did here and at Manchester; the observations taken on that day, will form the most valuable collection that has yet been made, from which to estimate the relative heights of their respective stations.

It may be remarked, however (and a similar remark was made by Mr. Bevan respecting his and Col. Beaufoy's observations for March and April), that on comparing Mr. Hanson's observations and my own for April, and those for the present month, it might be supposed that an addition had been made to the quantity of mercury in his barometer; or that a portion had been taken from that in mine: neither of which suppositions would be correct.

It is worthy of notice, also, that when Mr. Hanson took the twelve o'clock observation on Monday last, the mercury in the tube of his barometer had sunk 1·005 of an inch; whereas, my barometer remained perfectly steady until one, when it fell 1·002 of an inch only. This is the more extraordinary, as the distance between Crumpsall and Manchester is not great, and the state of the weather was much the same at both places.

A well conducted course of contemporaneous observations, made at a number of distant places, variously situated with regard to each other, would probably lead to a more accurate knowledge of the nature and operation of those causes, that occasion such frequent fluctuations in the weight of the atmosphere, and, consequently, in the length of the mercurial column by which those fluctuations are measured. I am, sir, your obed. servant,

To the Editor. John Blackwall.

Leighton, May 24, 1821.

Dear Sir,—I am sorry that it is not in my power this month, to send the barometrical observations at this place, owing to a mistake in the day, inadvertently concluding the 7th being the proper day, instead of the 14th; and not discovering my error till too late to supply the defect.

I have the pleasure to send you the observations made by Col. Beaufoy at Bushy-Heath, as below:

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	28·865	47·5	45	W.N.W.	fresh.	Fine.
9	28·869	47·3	46	W.N.W.	do.	Do.
10	28 867	47·3	49	W.N.W.	squally.	Cloudy.
11	28·868	48	49	W.	do.	Showery.
12	28·871	49·7	51	W.	do.	Do.
1	28·879	49·7	52	W.N.W.	do.	Do.

I have also the pleasure to send you the observations made by Mr. Cornfield, at Northampton, on the same day, by an instrument suspended in his lecture room, which has been ascertained to be about 39 feet above the surface of the canal above the bottom lock or entrance into the river Nene. The barometer of Mr. Cornfield I estimate to be about 63 feet lower than mine.

1821.	Barom.	Therm. att.	Therm. det.
8h 3m	29·172	53·6	53·6
9 3	29·186	59	48
10	29·188	55·6	54
11	29·194	56	52
12	29·192	56	55
1	29·199	57	56

I also subjoin the calculated results of the preceding six months from the observations made at Leighton and Bushy, rejecting fractions.

Bushy above Leighton.

			Feet.	Wind.
By observ. in	Nov. 1820	=	232	N.N.E.
	Dec. ..	=	210	S.W.
	Jan. 1821	=	233	E. by S.
	Feb. ..	=	228	N.E. by E.
	March ..	=	194	W. by S.
	April ..	=	222	S.S.W.

B. Bevan.

METEORO-

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS			
April 15	13	47·	29·30	Cloudy
16	14	54·	29·25	Fine
17	full	53·5	29·25	Ditto
18	16	56·	29·50	Ditto
19	17	52·5	29·36	Rain
20	18	59·	29·40	Fine
21	19	55·5	29·65	Ditto
22	20	50·	29·80	Ditto
23	21	56·	29·40	Cloudy
24	22	68·	29·20	Fine
25	23	71·5	29·35	Ditto
26	24	67·5	29·35	Ditto—lightning at night.
27	25	62·	29·40	Cloudy—rain A.M.
28	26	64·	29·55	Fine
29	27	55·	29·65	Cloudy
30	28	53·	29·80	Ditto
May 1	new	57·	29·80	Ditto
2	1	64·	29·60	Fine
3	2	64·	29·93	Ditto
4	3	69·5	29·36	Ditto—showery, with thunder and lightning P.M.
5	4	70·	29·23	Ditto
6	5	60·	29·15	Ditto
7	6	61·	29·18	Cloudy
8	7	62·	29·64	Fine
9	8	58·	29·86	Ditto—rain A.M.
10	9	57·	29·90	Ditto
11	10	60·	29·67	Cloudy
12	11	56·	29·50	Fine
13	12	48·	29·10	Showery
14	13	54·	29·08	Ditto

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For May 1821.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
April 27	57	66	55	29·80	Fair
28	57	67	55	·90	Fair
29	54	61	50	·92	Fair
30	50	50	46	30·10	Cloudy
May 1	44	52	50	·07	Cloudy
2	50	62	50	29·94	Cloudy
3	55	67	57	·79	Fair
4	56	69	60	·76	Fair
5	57	65	50	·63	Fair
6	50	59	50	·60	Stormy
7	51	60	56	·95	Showery
8	55	59	50	30·05	Rain
9	50	61	47	·25	Fair
10	51	59	51	·28	Cloudy
11	52	60	58	·05	Cloudy
12	58	61	52	29·90	Cloudy
13	52	57	42	·36	Stormy
14	46	55	47	·41	Fair
15	47	54	46	·30	Storms with thun-
16	50	57	47	·87	Stormy [der.
17	50	51	50	·98	Rain
18	50	60	49	30·13	Fair
19	51	61	50	·32	Fair
20	46	56	46	·26	Cloudy
21	45	55	45	·16	Cloudy
22	46	56	44	·02	Cloudy
23	43	47	42	29·83	Rain
24	44	50	43	30·07	Cloudy [evening.
25	47	57	41	29 89	Fair—rain in the
26	41	45	39	·83	Showers of hail and sleet.

N B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

14th May 8 o'Clock	M. Barom.	29·394	Ther. attached	55°	Detached	47
— — 9	— — —	29 410	— — —	55	— —	51
— — 10	— — —	29·410	— — —	55	— —	51
— — 11	— — —	29 410	— — —	55	— —	57
— — 1	— N. —	29 410	— — —	56	— —	55

LXX. *Description of a Lock designed for the* Regent's *Canal Company.* *By Mr.* Richard Hall Gower, *of Ipswich**.

Explanation.

The plan of a double lock, whereby *twice* the facility of transit is obtained, with only *half* the expenditure of water.

Example. A and B (Plate IV.) are locks having a communication by means of the sluices W and *x* in the middle pier. Now admitting lock A shall be full, and lock B empty, at the same time that two barges shall arrive, the one going down, and the other up the stream; the barge going down will naturally enter the lock A which is ready for her reception; while the other will enter B. The sluices and gates being now shut, let the middle pier sluices be opened, so that the water may flow from lock A into B (view the transverse section), whereby the barge in A will be lowered, and the barge in B raised, till both are on a level; at which time the barge in A will be half up, and the barge in B half down. Now shut the pier sluices W and *x*, and open the side sluices *y* and *z*, whereby lock A will continue to empty, and B to fill, till the water in each obtain the level of the lower and upper canal:—the gates C and D being then opened, each barge is at liberty to depart, the one up and the other down the stream; the time employed to pass them being no more than the time employed in passing *one* barge through a single lock; and to perform this double duty, only *one* full lock of water has been withdrawn from the upper level of the canal.

* Mr. Gower (author of several works relative to Seamanship and Marine affairs) was one of the candidates for the reward of a hundred guineas offered by the Company for the best design of a lock, in an advertisement of which the following is a copy:

"Regent's Canal.—To Architects, Engineers, &c. a reward of 100 guineas is offered for the best design of a double or single lock, to be constructed in the said canal. In these designs, part of the lock must be described and set forth. In judging of these designs, the saving of water, and the facility and expedition in passing the lock, will be objects of the greatest consideration; and any practicable suggestions for returning the water to the upper levels will have great weight in the decision. The length of the lock must be 86 feet, the breadth of each, in the clear, must be 14 feet 3 inches; the average fall of water of each lock 7 feet; the width of the canal 45 feet; the depth of the canal 5 feet.—Any person willing to execute his design, is requested to accompany it with an estimate of the lock complete, for so much each lock. The designs are to be submitted to three engineers or scientific men, and the premium given according to the decision of the majority of them; and to ensure perfect confidence in the decision, candidates are to send their designs, marked, to be returned unopened, if the design to which it refers is not approved, and should not obtain the premium. The designs to be sent to Messrs. Edwards and Lyon, Solicitors, Great Russell-street, Bloomsbury, on or before the 21st of September 1812."—*Times Newspaper, 5th Sept.* 1812.

The

TABLE continued.

Argument. Long. Eclip.	Signs 0.— and VI.— Reduction.	Diff.	Var. Obl.	Signs I.— and VII.— Reduction.	Diff.	Var. Obl.	Signs II.— and VIII.— Reduction.	Diff.	Var. Obl.	Argument. Long. Eclip.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
16 0	1 15 47·69	42·84	1·06	2 28 19·67	0·26	2·17	1 12 21·72	47·04	1·10	14 0
10	1 16 30·53	42·70	1·07	2 28 19·93	0·03	2·17	1 11 34·68	47·21	1·09	50
20	1 17 13·23	42·55	1·08	2 28 19·90	0·31	2·17	1 10 47·47	47·37	1·08	40
30	1 17 55·78	42·42	1·09	2 28 19·59	0·64	2·17	1 10 0·10	47·51	1·06	30
40	1 18 38·20	42·27	1·10	2 28 18·95	0·93	2·17	1 9 12·59	47·69	1·05	20
50	1 19 20·47	42·14	1·11	2 28 18·02	1·23	2·17	1 8 24·90	47·84	1·04	10
17 0	1 20 2·61	41·98	1·12	2 28 16·79	1·55	2·17	1 7 37·06	47·98	1·03	13 0
10	1 20 44·59	41·85	1·13	2 28 15·24	1·82	2·17	1 6 49·08	48·16	1·02	50
20	1 21 26·44	41·69	1·14	2 28 13·42	2·13	2·17	1 6 0·92	48·30	1·00	40
30	1 22 8·13	41·52	1·15	2 28 11·29	2·46	2·17	1 5 12·62	48·44	0·99	30
40	1 22 49·65	41·40	1·16	2 28 8·83	2·74	2·17	1 4 24·18	48·59	0·98	20
50	1 23 31·05	41·23	1·17	2 28 6·09	3·06	2·17	1 3 35·59	48·75	0·96	10
18 0	1 24 12·28	41·09	1·18	2 28 3·03	3·34	2·17	1 2 46·84	48·87	0·95	12 0
10	1 24 53·37	40·93	1·19	2 27 59·69	3·67	2·17	1 1 57·97	49·03	0·94	50
20	1 25 34·30	40·77	1·20	2 27 56·02	3·95	2·17	1 1 8·94	49·16	0·93	40
30	1 26 15·07	40·61	1·21	2 27 52·07	4·26	2·17	1 0 19·78	49·30	0·91	30
40	1 26 55·68	40·46	1·22	2 27 47·81	4·57	2·17	0 59 30·48	49·44	0·90	20
50	1 27 36·14	40·28	1·23	2 27 43·24	4·86	2·17	0 58 41·04	49·57	0·89	10
19 0	1 28 16·42	40·14	1·24	2 27 38·38	5·19	2·17	0 57 51·47	49·70	0·88	11 0
10	1 28 56·56	39·97	1·25	2 27 33·19	5·45	2·17	0 57 1·77	49·83	0·87	50
20	1 29 36·53	39·80	1·26	2 27 27·74	5·81	2·17	0 56 11·94	49·97	0·85	40
30	1 30 16·33	39·61	1·27	2 27 21·93	6·07	2·17	0 55 21·97	50·10	0·84	30
40	1 30 55·97	39·48	1·28	2 27 15·86	6·41	2·17	0 54 31·87	50·21	0·83	20
50	1 31 35·45	39·29	1·29	2 27 9·45	6·69	2·17	0 53 41·66	50·33	0·81	10
20 0	1 32 14·74	39·13	1·30	2 27 2·76	6·99	2·17	0 52 51·33	50·46	0·80	10 0
10	1 32 53·87	38·97	1·31	2 26 55·77	7·52	2·16	0 52 0·87	50·58	0·79	50
20	1 33 32·84	38·80	1·32	2 26 48·45	7·57	2·16	0 51 10·2[illegible]	50·70	0·77	40
30	1 34 11·64	38·61	1·33	2 26 40·8[illegible]	7·93	2·16	0 50 19·59	50·81	0·76	30
40	1 34 50·25	38·45	1·34	2 26 32·9[illegible]	8·21	2·16	0 49 28·78	50·93	0·75	20
50	1 35 28·70	38·27	1·35	2 26 24·74	8·51	2·16	0 48 37·83	51·03	0·74	10
21 0	1 36 6·97	38·09	1·36	2 26 16·23	8·83	2·15	0 47 46·82	51·15	0·73	9 0
10	1 36 45·06	37·91	1·37	2 26 7·40	9·11	2·15	0 46 55·67	51·25	0·71	50
20	1 37 22·97	37·74	1·38	2 25 58·29	9·43	2·15	0 46 4·42	51·37	0·70	40
30	1 38 0·71	37·55	1·39	2 25 48·86	9·72	2·15	0 45 13·05	51·46	0·69	30
40	1 38 38·26	37·39	1·40	2 25 39·14	10·07	2·15	0 44 21·59	51·57	0·67	20
50	1 39 15·65	37·18	1·41	2 25 29·07	10·33	2·15	0 43 30·02	51·67	0·66	10
22 0	1 39 52·83	37·00	1·42	2 25 18·74	10·62	2·15	0 42 38·35	51·76	0·65	8 0
10	1 40 29·83	36·81	1·43	2 25 8·12	10·96	2·14	0 41 46·59	51·86	0·64	50
20	1 41 6·64	36·64	1·44	2 24 57·16	11·23	2·14	0 40 54·73	51·96	0·62	40
30	1 41 43·28	36·43	1·45	2 24 45·93	11·55	2·14	0 40 2·77	52·04	0·61	30
40	1 42 19·71	36·26	1·45	2 24 34·38	11·85	2·14	0 39 10·73	52·14	0·60	20
50	1 42 55·97	36·08	1·46	2 24 22·53	12·16	2·14	0 38 18·59	52·23	0·58	10
23 0	1 43 32·05	35·87	1·47	2 24 10·37	12·44	2·13	0 37 26·36	52·31	0·57	7 0
10	1 44 7·92	35·65	1·48	2 23 57·93	12·77	2·13	0 36 34·05	52·40	0·55	50
20	1 44 43·57	35·50	1·49	2 23 45·16	13·07	2·13	0 35 41·65	52·48	0·54	40
30	1 45 19·07	35·29	1·50	2 23 32·09	13·37	2·13	0 34 49·17	52·56	0·53	30
40	1 45 54·36	35·08	1·50	2 23 18·72	13·65	2·13	0 33 56·61	52·64	0·51	20
50	1 46 29·44	34·91	1·51	2 23 5·07	13·95	2·12	0 33 3·97	52·70	0·50	10
	Signs V.+ and XI.+			Signs IV.+ and X.+			Signs III.+ and IX.+			

out the comfort even of an explanation. he is simply told that the premium of 100 guineas was not adjudged to him.

Letter addressed by Mr. GOWER *to the* REGENT'S *Canal Company.*

Nova Scotia, Ipswich, Oct. 19, 1820.

GENTLEMEN,—On the 5th of September 1812, observing an advertisement in the Times Newspaper, from the Regent's Canal Company, offering a reward of 100 guineas for the best design of a lock to *save* water, and give *facility* to passage, a copy of which is annexed, I was induced to become a candidate.

A design was accordingly prepared, *similar to the one which accompanies this letter*, being dated the 14th of September 1812; and feeling, from the wording of the advertisement, that I might rely with perfect confidence upon the good faith and honour of the parties to whose inspection the designs were to be submitted, it was delivered at the office of Messrs. Edwards and Lyon, Solicitors, Great Russell-street, Bloomsbury, by my brother Dr. Gower, of Old Burlington-street. A fair time having elapsed without receiving any intimation relative to the design, from the tenor of the advertisement, I had reason to conclude it was not approved; and in consequence requested Dr. Gower would have the goodness to withdraw it. He made his application accordingly, at the office of Messrs. Edwards and Lyon, which enabled him to procure the design from the Regent's Canal Office, Queen Ann's-street West, now Foley-place.

The return of the design released me from dubiety. It was the signal of dismission, that gave me clearly to understand I was not the successful candidate. How great then was my surprise, when on the 24th of April last, as travelling from Ipswich to London, on the outside of the coach, I observed a double lock, similar to my design, constructed on the Regent's Canal where it crosses the Mile-end Road! This was afterwards found to be one only of a number. From the facts here stated, and with the Company's advertisement before me, I cannot but feel that I have a claim upon their honour, and that I am entitled to the reward for which I laboured to become a candidate; I therefore trust, gentlemen, notwithstanding the time elapsed since the 14th of September 1812, that you will do me the justice to consider this statement; and should it be necessary for Dr. Gower to prove the delivery of the drawing, he will with pleasure attend with the original design.

I remain, gentlemen,

Your obedient servant,

R. H. GOWER.

Reply of the Regent's Canal Company to the foregoing Application.

Regent's Canal Office, 108, Great Russell-street, Oct. 26, 1820.

SIR,—I am directed to acknowledge the receipt of your letter of the 19th instant, stating that the locks on the Regent's Canal had been constructed according to the design sent in by you, in the year 1812, and claiming the premium of 100 guineas offered for the best design for a lock; and in reply I am desired to acquaint you, that the engineers to whom the several designs were referred, did not adjudge the design sent in by you to be entitled to the premium above mentioned; and that no reference was had to your plans, on the adoption of the lock which the Company determined to construct upon the Regent's Canal.

I am, sir,

Your obedient servant,

EDM. L. SNEE.

P. S.—The plan which accompanied your letter, will be delivered to any person bringing a note from you desiring the return of the same.

LXXI. *On the atmospherical Refraction.* *By* JAMES IVORY, *M.A. F.R.S.*

To the Editor.

SIR,—I HAVE to acknowledge the favour done me by your inserting in your last publication, my formula for the astronomical Refractions; and I shall now add some more observations on the same subject. In examining the hypothesis relating to the constitution of the atmosphere, on which is founded the solution of the problem of refractions published in the *Mécanique Céleste*, I was naturally led to take the calculation, given in that work itself, of the depression of the thermometer for the great height of 3817 fathoms ascended by Gay-Lussac in a balloon; which comes out at the rate of 82 fathoms to a centesimal degree. But the gradation of heat in this supposed atmosphere ought to agree with observation not only at great elevations, but likewise at the surface of the earth. Now, if the calculation be actually made, the height at the earth's surface necessary to depress the thermometer one degree, will be found no more than 61 fathoms, about two-thirds of the observed quantity, and considerably less than in my formula.

We are indebted for the most correct Table of Refractions that has yet been produced, to the science of Laplace, and the skill and diligence of the able men who assisted his labours by furnishing

nishing him with exact determinations of the necessary constant quantities. As far as 80° from the zenith, or even as far as 85°, a greater degree of perfection can hardly be expected in this part of astronomy. But within 5° of the horizon, the Table is confessedly incorrect in theory, and does not agree so well with observation. The author of the *Mécanique Céleste*, by a single artifice of calculation, both overcame the analytical difficulties of the problem, and hit upon a law of the decrease of heat, which affords a determination of the quantity sought, simple in its expression and approaching very near the truth. But the constitution of the atmosphere thus adopted deviates from the case of nature in a twofold manner: first, the initial rate of the decrease of heat is too great; secondly, the rate of decrease becomes slower as the elevation increases; whereas, in nature, it is either uniform or accelerated. By means, however, of this double departure from the true law, a compensation is effected to which we must, in a good measure, ascribe the great exactness of the result. For in the atmosphere of Laplace the heat, at any given elevation, is at first greater than in the case of nature; but, as the rate of decrease continually lessens, the former quantity approaches the latter as the height increases, and, at a certain elevation, will coincide with it; beyond which limit the heat in the supposed atmosphere will become less than the true quantity, as at first it was greater.

In my formula there is no hypothesis implied, except that of a uniform decrease of heat. The expression of the refraction is a converging series rigorously integrated upon that hypothesis. The elevation necessary for depressing the thermometer one degree depends upon the magnitude of the horizontal refraction; and a very small addition to the latter quantity would make my theory agree entirely with observation. Now there are good grounds for thinking that the horizontal refraction in the French Tables is rather too small. In the first place, when these Tables are compared with exact observations near the horizon, the errors in defect are found to be nearly double the errors in excess *: in the second place, the refraction of the star *Lyra*, at 87° 42′ 10″ from the zenith, is determined, by the observations of Mr. Brinkly, to be 17′ 26″·5, or 5″ more than according to the same Tables †: and thirdly, in the Table published by Mr. Groombridge from his own observations, all the refractions very near the horizon are greater than in the French Tables; particularly the horizontal refraction is 34′ 28″·13 in place of 33′ 46″·3. ‡ The determination of Mr. Groombridge appears to be fully sufficient to make my theory quadrate entirely with observation as to the constitu-

* *Conn. des Tems*, 1821, p. 349. † Irish Trans. 1815.
‡ Phil. Trans. 1814.

tion

tion of the atmosphere: for, I find that the refractions at the horizon depend not only upon the initial rate of the decrease of heat, but also, although in a very small degree, upon the acceleration.

Since my last communication, I have applied to my method an artifice similar to that employed by Laplace for integrating the expression of the refraction near the horizon. The result is contained in the following formula, the letters denoting the same things as before:

$$\text{Log. tan } \varphi = 19{\cdot}0217998 - \text{log. cos. } A\text{; then,}$$

$$R = \sin \times \begin{cases} 1109''{\cdot}26 \tan \tfrac{1}{2}\varphi & \ldots\ \ldots & 3{\cdot}0450325 \\ 665{\cdot}55 \tan^3 \tfrac{1}{2}\varphi & \ldots\ \ldots & 2{\cdot}8231838 \\ 221{\cdot}85 \tan^5 \tfrac{1}{2}\varphi & \ldots\ \ldots & 2{\cdot}3460625 \\ 31{\cdot}69 \tan^7 \tfrac{1}{2}\varphi & \ldots\ \ldots & 1{\cdot}5009645. \end{cases} \quad \text{(Log. of Coefficients.)}$$

Owing to the artifice employed in the integration, this formula is a finite expression. It supposes an atmosphere similar to that of Laplace; that is, one in which the initial rate of the decrease of heat is too great, but continually becoming less and less as the elevation increases. It is reduced to the same state of the barometer and thermometer as in the French Tables. To the extent of 85° from the zenith, it does not differ sensibly from those Tables, nor from the former formula: for less altitudes it comes a little nearer the Tables, as is shown below.

A.	Conn. des T.	Formula.	Diff.
°	′ ″	′ ″	″
85	9 54·3	9 53·4	−0·9
86	11 48·3	11 46·7	−1·6
87	14 28·1	14 25·4	−2·7
88	18 22·2	18 16·8	−5·4
89	24 21·2	24 12·6	−8·6
89½	28 32·1	28 24·3	−7·8
90	33 46·3	33 48·3	+2·0

I have the honour to be, &c.

June 6, 1821. J. Ivory.

LXXII. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1821. *By the Rev.* J. Grooby.

[Continued from p. 355.]

1821.

1821.	γ Pegasi.	α Arietis	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 52	H. M. 4 25	H. M. 5 3	H. M. 5 5	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 39	H. M. 11 41	H. M. 13 15	H. M. 14 7
Aug.	s	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	4·87	8·85	58·20	41·57	30·99	57·82	0·99	30·63	16·31	11·52	56·96	22·64	48·45	51·28	57·11	23·02	48·49	32·33
2	90	88	23	60	31·03	84	1·02	65	33	54	98	66	46	28	10	01	48	32
3	92	91	26	63	07	87	05	68	35	57	57·00	68	47	29	10	01	47	31
4	95	94	29	66	11	90	08	71	38	59	02	70	48	29	09	01	46	29
5	98	98	32	69	15	93	11	73	40	61	04	72	49	29	08	00	45	28
6	5·00	9·01	35	72	19	95	15	76	42	63	05	74	49	30	08	00	44	27
7	03	04	38	75	23	98	18	78	44	65	07	76	50	30	07	22·99	43	25
8	05	08	41	78	26	58·01	21	81	46	68	09	78	51	31	07	99	42	24
9	08	11	44	82	31	04	24	84	49	70	11	81	52	31	07	99	41	22
10	10	14	47	85	35	07	28	87	51	73	13	83	53	32	06	98	40	21
11	12	17	50	88	39	10	31	90	53	75	15	85	54	32	06	98	39	20
12	14	20	53	91	44	12	34	92	56	78	17	88	55	33	06	98	38	18
13	16	23	56	94	48	15	38	95	58	80	19	90	56	34	06	97	38	17
14	19	26	59	98	52	18	41	98	60	83	21	92	57	34	05	97	37	16
15	21	30	62	42·01	56	21	44	31·01	63	85	24	95	58	35	05	97	36	14
16	23	33	65	04	61	23	48	03	65	88	26	97	59	36	05	97	35	13
17	25	36	68	07	65	26	51	06	67	90	28	99	60	36	05	96	34	11
18	27	39	71	10	69	29	54	09	69	93	30	23·01	61	37	04	96	33	10
19	29	42	74	14	73	32	58	12	72	96	32	04	62	38	04	95	32	08
20	31	45	77	17	77	35	61	15	74	98	34	07	64	39	04	94	31	07
21	33	48	80	20	82	38	65	18	76	12·01	37	09	65	40	04	93	31	06
22	35	51	83	23	86	41	68	20	79	04	39	11	66	41	04	92	30	04
23	37	53	86	26	90	43	71	23	81	06	41	14	67	42	04	91	29	03
24	39	56	88	29	94	46	75	26	83	09	43	16	69	43	04	90	28	02
25	41	59	91	32	98	49	78	29	86	12	45	19	70	44	04	89	27	00
26	43	62	94	35	32·02	52	82	31	88	14	47	21	71	45	04	88	27	31·99
27	45	65	97	38	06	55	85	34	90	17	50	24	72	46	04	87	26	98
28	47	68	59·00	41	11	58	88	35	93	20	52	27	74	48	04	86	25	97
29	49	71	03	44	15	61	91	37	95	22	54	30	75	49	04	85	24	95
30	51	74	06	47	19	64	94	38	97	25	57	33	76	50	04	84	24	94
31	53	77	09	49	23	67	97	40	99	28	59	36	78	51	04	83	23	93

1821.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Aug.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	50·77	2·21	9·37	30·26	29·29	32·52	40·99	55·83	48·51	6·52	34·84	47·13	10·01	23·09	38·87	48·36	54·38	12·32
2	76	20	35	25	28	51	98	82	51	52	84	14	02	09	88	38	40	35
3	75	19	34	24	27	50	97	81	52	52	85	14	02	10	90	40	42	38
4	73	17	32	22	25	49	96	80	52	53	85	15	03	10	92	42	44	41
5	72	16	31	21	24	48	95	79	52	53	85	16	04	11	93	44	46	44
6	71	15	30	20	23	47	94	78	52	53	85	16	04	11	95	46	48	46
7	70	14	28	19	22	46	93	77	53	54	86	17	05	11	96	49	50	49
8	69	13	27	18	21	45	92	76	53	54	86	17	05	12	98	51	52	52
9	68	12	25	16	20	44	91	75	53	54	86	17	05	12	99	53	54	54
10	67	11	23	15	19	43	90	73	53	54	86	17	05	12	39·00	55	56	57
11	66	10	22	14	17	41	89	72	53	54	86	18	06	11	01	56	57	59
12	64	08	20	13	16	40	88	71	53	53	86	18	06	11	02	58	59	61
13	63	07	18	11	15	39	87	69	52	53	86	18	06	11	03	60	61	64
14	62	06	16	10	14	37	85	68	52	53	86	18	06	11	04	62	62	66
15	60	04	14	09	12	36	84	67	52	52	86	19	07	10	06	64	64	68
16	59	03	13	08	11	35	83	65	52	52	85	19	07	10	07	65	66	70
17	58	02	11	06	10	33	82	64	51	52	85	19	07	10	08	67	67	73
18	56	·00	09	05	09	32	81	63	51	52	85	19	07	09	09	69	69	75
19	55	1·99	07	03	07	31	79	61	51	51	84	19	07	09	10	70	70	77
20	54	98	05	02	06	29	78	59	50	51	84	18	06	08	10	72	72	79
21	53	97	04	01	04	28	77	57	50	50	83	18	06	07	11	73	73	81
22	52	96	02	29·99	03	26	75	56	49	50	83	18	06	06	12	74	74	83
23	50	94	00	98	01	25	74	54	49	49	82	18	06	06	12	75	75	85
24	49	93	8·98	96	00	24	73	52	48	49	82	17	05	05	13	77	76	87
25	48	92	96	95	28·99	22	71	50	48	48	81	17	05	04	14	78	78	89
26	47	91	95	94	97	21	70	49	47	48	81	17	05	03	15	79	79	91
27	46	90	93	92	96	19	68	47	47	47	80	16	04	03	15	81	80	93
28	44	88	91	90	95	18	67	45	46	47	79	16	04	02	16	82	81	95
29	43	87	90	89	93	16	65	43	45	46	78	15	03	01	16	83	82	97
30	42	86	88	87	91	15	64	41	44	45	78	15	03	00	17	84	83	98
31	41	85	86	86	90	13	63	39	43	44	77	14	02	22·98	17	84	84	13·00

LXXIII. *On Light.* *By* Andrew Ure, *M.D. Professor of the Andersonian Institution, Glasgow* *.

Light.—The agent of vision.—Some philosophers regard light as consisting of particles of inconceivable minuteness, emitted in succession by luminous bodies, which move in straight lines, at the rate of 200,000 miles per second.

Others conceive that it consists in certain undulations communicated by luminous bodies, to an ethereal fluid which fills all space. This fluid is composed of the most subtile matter, is highly elastic, and the undulations are propagated through it with great velocity, in spherical superficies proceeding from a centre. This view derives great plausibility from its happy application by Huygens, to explain a very difficult class of optical phænomena, the double refraction of calcareous spar and other bodies.

The *common* refraction is explained by Huygens on the supposition, that the undulations in the luminous fluid are propagated in the form of *spherical* waves. The *double* refraction is explained on the supposition, that the undulations of light, in passing through the calcareous spar, assume a *spheroidal* form; and this hypothesis, though it does not apply with the same simplicity as the former, yet admits of such precision, that a proportion of the axes of the spheroids may be assigned, which will account for the precise quantity of the extraordinary refraction, and for all the phænomena dependent on it, which Huygens had studied with great care, and had reduced to the smallest number of general facts.

"That these spheroidal undulations actually exist," says the celebrated Playfair, "he would after all be a bold theorist who should affirm; but that the supposition of their existence is an accurate expression of the phænomena of double refraction, cannot be doubted. When one enunciates the hypothesis of the spheroidal undulations, he in fact expresses in a single sentence all the phænomena of double refraction. The hypothesis is therefore the means of representing these phænomena, and the laws which they obey, to the imagination or the understanding; and there is perhaps no theory in optics, and but very few in natural philosophy, of which more can be said. Theory therefore, in this instance, is merely to be regarded as the expression of a general law, and in that light I think it is considered by Laplace."

Dr. Young has selected from Sir Isaac Newton's various writings, many passages favourable to the admission of the undulatory theory of light, or of a luminiferous ether pervading the universe,

* From Ure's Dictionary of Chemistry.

verse, rare and elastic in a high degree. "Is not the heat (of the warm room) conveyed through the vacuum by the vibrations of a much subtiler medium than air? And is not this medium the same with that medium by which light is reflected and refracted, and by whose vibrations light communicates heat to bodies, and is put into fits of easy reflection and easy transmission? And do not the vibrations of this medium in hot bodies contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium, propagated from them into the cold ones? And is not this medium exceedingly more rare and subtile than the air, and exceedingly more elastic and active? And doth it not readily pervade all bodies? And is it not by its elastic force expanded through all the heavens?"—"If any one would ask how a medium can be so rare, let him tell me how an electric body can by friction emit an exhalation so rare and subtile, and yet so potent? and how the effluvia of a magnet can pass through a plate of glass without resistance, and yet turn a magnetic needle beyond the glass?"—*Optics*, *Qu.* 18, 22. "Were I to assume an hypothesis, it should be this, if propounded more generally, so as not to determine what light is, further than that it is something or other capable of exciting vibrations in the ether; for thus it will become so general, and comprehensive of other hypotheses, as to leave little room for new ones to be invented."—*Birch*, iii. 249.

Dr. Young shows, that many phænomena inexplicable on the notion of radiating corpuscles, are easily reconciled to the theory of undulation. "On the whole," says this profound philosopher, "it appears that the few optical phænomena, which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others which have been long known, but never understood, become by these means perfectly intelligible; and that several new facts are found to be thus, only, reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system."—*Nat. Phil.* vol. ii. p. 631.

I think, however, that the new discoveries on polarized light may be more easily referred to the corpuscular than undulatory hypothesis.

The physical affections of light are foreign to this work. Its chemical relations are alone to be considered. These may be conveniently referred to four heads:

1. Of the mean refractive and dispersive powers of different bodies.

2. Of the action of the different prismatic colours on chemical matter.

3. Of the polarization of light.

4. Of

4. Of the absorption and disengagement of light or phosphorescence.

1. Newton first discovered that certain bodies exercise on light a peculiar attractive force. When a ray passes obliquely from air into any transparent liquid or solid surface, it undergoes at entrance an angular flexure, which is called *refraction*. The variation of this departure from the rectilineal path for any particular substance, depends on the obliquity of the ray to the refracting surface ; so that the sine of the angle of refraction, is to that of the angle of incidence, in a constant ratio. Now Newton found, that unctuous or inflammable bodies occasioned a greater deviation in the luminous rays than their attractive mass or density gave reason to expect. Hence he conjectured, that both diamond and water contained combustible matter,—a sagacious anticipation of future chemical discovery.

Dr. Wollaston invented a very ingenious apparatus, in which, by means of a rectangular prism of flint glass, the index of refraction of each substance is read off at once by a vernier, the three sides of a moveable triangle performing the operations of reduction, in a very compendious manner.—*Phil. Trans.* 1802, or *Nicholson's Journal*, 8vo. vol. iv. p. 89.

But transparent media occasion not merely a certain flexure of the white sunbeam, called the *mean refraction*, they likewise decompose it into its constituent colours. This effect is called *dispersion*. Now the mean refractive and dispersive powers of bodies are not proportional to each other. In some refracting media, the mean angle of refraction is larger, whilst the angle of dispersion is smaller ; and in other refracting media, the mean angle of refraction is smaller, whilst the angle of dispersion is larger. In short, the knowledge of the mean refractive power of a given substance, will not enable us to determine its dispersive power, and *vice versâ*.

From the refractive power of bodies we may in many cases infer their chemical constitution. For discovering the purity of essential oils, an examination with Dr. Wollaston's instrument may be of considerable utility, on account of the smallness of the quantity requisite for trial. "In oil of cloves, for instance, I have met with a wide difference. The refractive power of genuine oil of cloves is as high as 1·535 ; but I have also purchased oil by this name which did not exceed 1·498, and which had probably been adulterated by some less refractive oil." This fine idea, suggested by Dr. Wollaston, has been happily prosecuted by M. Biot, with regard to gaseous compounds. I shall first give general tables of the refractive and dispersive powers of different bodies, and then make some remarks on their chemical applications:

		Index of Refraction.
A vacuum,		1·00000
Atmospheric air, (mean,)		1·00033
Ice,	W.	1·31000
Ice,	Brewster,	1·30700
Water,		1·336
Vitreous humour, Cryolite.	B.	1·344
Ether,	Wol.	1·358
Albumen,	W.	1·360
Alcohol,	W.	1·370
Saturated solution of salt,	Cavallo,	1·375
Solution of sal ammoniac,		1·382
Nitric acid, sp.gr. 1·48,	W.	1·410
Fluor spar,	W.	1·433
Sulphuric spar,	W.	1·435
Spermaceti melted,	W.	1·446
Crystalline lens of an ox,	W.	1·447
Alum,	W.	1·457
Tallow melted,	W.	1·460
Borax,	C.	1·467
Oil of lavender,	W.	1·467
	C.	(1·469)
Oil of peppermint,	W.	1·468
Oil of olives,	W.	1·469
Oil of almonds,	W.	1·470
Oil of turpentine, rectified,	W.	1·470
Do. common,	W.	1·476
Essence of lemon,	W.	1·476
Butter, cold,	W.	1·480
Linseed oil,	W.	1·485
Camphor,	W.	1·487
Iceland spar, weakest refr.	W.	1·488
Do. strongest do.	W.	(1·657)
Tallow, cold,	W.	1·49
Sulphate of potash,	W.	1·495
Oil of nutmeg,	W.	1·497
French plate-glass,	W.	1·500

		Index of Refraction.
English plate-glass,	W.	1·504
Oil of amber,	W.	1·505
Balsam of capivi,	W.	1·507
Gum-arabic,	W.	1·514
Dutch plate-glass,	W.	1·517
Caoutchouc,	W.	1·524
Nitre,	C.	1·524
Selenite,	W.	1·525
Crown glass, com.	W.	1·525
Canada balsam,	W.	1·528
Centre of the crystalline of fish, and dry crystalline of an ox,	W.	1·530
Pitch,	W.	
Radcliffe crown-glass,	W.	1·533
Anime,	W.	1·535
Copal,	W.	1·535
Oil of cloves,	W.	1·535
White wax, cold, Elemi, Mastic, Arseniate of potash, Sugar after fusion, Spermaceti, cold, Red sealing-wax,	W.	
Oil of sassafras,	W.	1·536
Bees-wax,	W.	1·542
Boxwood,	W.	
Colophony,	W.	1·543
Old plate-glass,	W.	1·545
Rock crystal, (double,)	W.	1·547
Amber,	W.	1·547
	C.	(1·556)
Opium,	W.	
Mica,	W.	
Phosphorus,	W.	1·579
Horn,	W.	
Flint-glass,	W.	1·583 1·586
Benzoin,	W.	
Guaiacum,	W.	1·596
Balsam of Tolu,	W.	1·600

Sul-

	Index of Refraction.		Index of Refraction.
Sulphate of barytes, (double R.)	W. 1·646	Jargon,	W. 1·950
Iceland spar, (strongest,)	W. 1·657	Glass of antimony,	W. 1·980
Gum dragon,	W.	Native sulphur,	W. 20·40
Carburet of sulphur,	Br. 1·680	Do.	Brewster, 2·115
White sapphire,	W. 1·768	Plumbago,	W.
Muriate of antimony, variable,	W.	Phosphorus,	Brewster, 2·224
Arsenic, (a good test,)	W. 1·811	Diamond, Newton, by Dr. W.	2·440
Spinelle ruby,	W. 1·812	Do.	Rochon, 2·755
		Realgar,	Brewster, 2·510
		Chromate of lead, (least refr.)	do. 24·79
		Do. (greatest refr.)	do. 2·926

TABLE II. — *Refracting Powers of Gases for the Temperature of* 32° F. *and Pressure* 30, *by MM.* BIOT *and* ARAGO.

Atmospheric air,	1·00000	Carbonic acid, ..	1·00476
Oxygen, ...	0·86161	Subcarburetted hydrogen,	2·09270
Azote, .. .	1·03408	Muriatic acid gas,	1·19625
Hydrogen, ..	6·61436		
Ammonia, ..	2·16851		

TABLE III.—*Dispersive Powers.*

Cryolite,	Brewster, 0·022	Sulphur,	Brewster, 0·130
Fluor spar,	do. 0·022	Oil of cassia, ..	do. 0·139
Water, ..	do. 0·035	Realgar, ..	do. 0·255
Diamond,	do. 0·038	Chromate of lead, (least refr.)	do. 0·262
Flint glass, (highest,)	do. 0·052	Do. (greatest refr.)	do. 0·400
Carburet of sulphur,	do. 0·115		
Phosphorus,	do. 0·128		

Carburet of sulphur exceeds all fluid bodies in refractive power, surpassing even flint-glass, topaz, and tourmaline; and in dispersive power it exceeds every fluid substance, except oil of cassia, holding an intermediate place between phosphorus and balsam of Tolu.

Dr. Brewster has further shown, that all doubly refracting crystals have two dispersive powers.

From Table II. it appears, that the refractive power of hydrogen gas greatly surpasses not only that of the other gases, but of all known bodies. This principle exists in great abundance, in resins, oils, and gums, where it is united to carbon and oxygen; and we must probably ascribe to it, the eminent refractive power of these combustibles, so justly observed by Newton. This effect of hydrogen is finely displayed in ammonia, whose refractive power

is more than double that of air; and much superior to that of water.

But since every substance ought to introduce into its combinations, its peculiar character, and preserve in them, to a certain degree, the force with which it acts on light, let us endeavour to calculate, in this point of view, the refractive influence of the constituents of a compound. From our knowledge of the extreme tenuity of light, it is probable, that the influence of a moderate chemical condensation, ought to affect its operations very slightly; for, whether it be an ether or a corpuscular emanation, the excessive minuteness of its particles, compared to the distances between the molecules of bodies, ought to render the change of distance among the latter, unimportant. Consequently, the refracting powers of bodies ought to differ very little from those of their elements, unless a very great degree of condensation has taken place.

Hence, if we multiply the proportions of azote and oxygen respectively, by their refractive powers, we shall obtain products, whose sums will coincide with the refractive power of the atmosphere. Thus, 100 parts by weight of the atmosphere, consist of azote 77·77 + oxygen 22·22. If we multiply each of these numbers by the number representing the refractive power of the body, and making a small correction for the carbonic acid present, we shall have for the sum of the products 1·0000.

Ammonia, however, furnishes a more interesting example of the application of these principles.

The refractive power of hydrogen is	.. ..	6·61436
	of azote ..	1·03408
	of ammonia	2 16851

Let x be the weight of the constituent, whose refractive power is .. .. a

$y = 100 - x =$ that whose power is .. .. b

and call the refractive power of the compound .. c

Then $x = \frac{c-b}{a-b}$. In the present case,

$$x = \frac{2{\cdot}16851 - 1{\cdot}03408}{6{\cdot}61436 - 1{\cdot}03408} = 0{\cdot}203 \text{ and } 100 - x = 0{\cdot}797 =$$

the azote in 100 parts of ammonia; which may be regarded as an approximation. The true proportions given by the equivalent ratios are, 0·823 azote + 0.177 hydrogen. If the refractive power of ammonia were 2·0218, then the chemical and optical analysis would coincide.

If we calculate on the above data, what ought to be the refractive power of water, as a compound of 8 parts of oxygen + 1 hydrogen, we shall obtain the number 1·50065, which being

multiplied

multiplied by 0·45302, the absolute refractive power of air, when we take the density of water for unity, we shall have a product =0·67984. Now, according to Newton's estimate, which M. Biot has found to be exact, the refractive power of water is 0·7845. Hence we see that the compound has acquired an increased refractive force by condensation, above the mean of its constituents, in the ratio of 100 to 86⅔.

Rays of light, in traversing the greater number of crystallized bodies, are commonly split into two pencils; one of which, called the ordinary ray, follows the common laws of refraction, agreeably to the preceding tables, whilst the other, called the extraordinary ray, obeys very different laws. This phænomenon is produced in all transparent crystals, whose primitive form is neither a cube nor a regular octohedron. The division of the beam is greater or less, according to the nature of the crystal, and the direction in which it is cut. But of all known substances, that which produces this phænomenon in the most energetic manner, is the rhomboidal carbonate of lime commonly called island spar.

Of the action of the different coloured *rays*. If the white sunbeam, admitted through a small hole of a window-shutter into a darkened room, be made to pass through a triangular prism of glass, it will be divided into a number of splendid colours which may be thrown upon a sheet of paper. Newton ascertained, that if this coloured image, or spectrum as it is called, be divided into 360 parts; the red will occupy 45, the orange 27, the yellow 48, the green 60, the blue 60, the indigo 40, and the violet 80. The red rays being least bent by the prism, from the direction of the white beam, are said to be least refracted, or the least refrangible; while the violet rays being always at the other extremity of the spectrum, are called the most refrangible. According to Dr. Wollaston, when the beam of light is only 1-20th of an inch broad, and received by the eye at the distance of 10 feet, through a clear prism of flint-glass, only four colours appear; red, yellowish-green, blue, and violet.

If the differently coloured rays of light thus separated by the prism, be concentred on one spot by a lens, they will reproduce colourless light. Newton ascribes the different colours of bodies, to their power of absorbing all the primitive colours, except the peculiar one which they reflect, and of which colour they therefore appear to our eye.

According to Sir William Herschel, the different coloured rays possess very different powers of illumination. The lightest green, or deepest yellow, which are near the centre, throw more light on a printed page than any of the rays towards either side of the spectrum. Sir H. Davy remarks, that as there are *more* green rays in a given part of the spectrum than blue rays, the difference

of

of illuminating power may depend on this circumstance. The rays separated by one prism, are not capable of being further divided by being passed through another; and in their relations to double refraction and reflection, they appear to agree with direct light. An object illuminated by any of the rays in the spectrum, is seen double through island crystal, in the same manner as if it had been visible by white light.

Under CALORIC, we have stated the power of heating which the different coloured rays of the spectrum apparently possess. Sir H. Englefield, and M. Berard, confirmed the results of Sir W. Herschel, with regard to the progressive increase of calorific influence from the violet to the red extremity of the spectrum; and they also found with him, that a calorific influence extended beyond the limit of the red light, into the unilluminated space. M. Berard, however, observed, that the maximum of effect was *in* the light, and not *beyond* it. This ingenious philosopher made a pencil of the sunbeam pass across a prism of island spar. The division of the rays formed two *spectra*, which presented the same properties with the single spectrum. Both possessed the calorific virtue in the same manner and degree. M. Berard polarized a beam of light by reflection from a mirror; and he found that in all the positions in which light ceased to be reflected, heat also ceased to appear. The thermometer in the focus of the apparatus was no longer affected. Thus, we see that the obscure heat-making principle accompanies the luminous particles, and obeys the same laws of action.

If the white *luna cornea*, the muriate of silver moistened, be exposed to the different rays in the prismatic spectrum, it will be found, that no effect is produced upon it, in the least refrangible rays, which occasion heat without light; that only a slight discoloration will be occasioned by the red rays; that the blackening power will be greater in the violet than in any other ray; and that beyond the violet, in a space perfectly obscure to our eyes, the darkening effect will be manifest on the muriate.

This fine observation, due to M. Ritter and Dr. Wollaston, proves, that there are rays more refrangible than the rays producing light and heat. As it appears, from the observations of M. Berthollet, that muriatic acid gas is formed when horn-silver is blackened by light, the above rays may be called hydrogenating. Sir H. Davy found, that a mixture of chlorine and hydrogen acted more rapidly upon each other, combining without explosion, when exposed to the red rays, than when placed in the violet rays; but that solution of chlorine in water became solution of muriatic acid most rapidly, when placed in the most refrangible rays in the spectrum. He also observed, that the puce-coloured oxide of lead, when moistened, gradually gained a tint of red in the

least

least refrangible rays, and at last became black, but was not affected in the most refrangible rays. The same change was produced by exposing it to a current of hydrogen gas. The oxide of mercury from calomel and water of potash, when exposed to the spectrum, was not changed in the most refrangible rays, but became red in the least refrangible; which must have been owing to the absorption of oxygen. The violet rays produced upon moistened red oxide of mercury, the same effect as hydrogen gas.

Dr. Wollaston found, that guaiac, exposed to the violet rays, passed rapidly from yellow to green; and MM. Gay Lussac and Thenard applied to the same influence a gaseous mixture of hydrogen and chlorine; when explosion immediately took place. By placing small bits of card, coated with moist horn-silver, or little phials of those mixed gases, in the different parts of the spectrum, M. Berard verified the former observations of the chemical power acquiring a maximum in the violet ray, and existing even beyond it; but he also found, that by leaving the tests a sufficient time in the indigo and blue rays, a perceptible effect was produced upon them. He concentrated by a lens all that portion of the spectrum which extends from the green to the extreme boundary of the violet; and by another lens he collected the other half of the spectrum, comprehending the red. The latter formed the focus of a white light, so brilliant, that the eye could not endure it; yet in two hours it produced no sensible change on muriate of silver. On the contrary, the focus of the other half of the spectrum, whose light and heat were far less intense, blackened the muriate in ten minutes. The investigations of Delaroche enable us, in some measure, to reduce these dissimilar effects of light to a common principle. See Caloric.

In Mr. Brande's late Bakerian lecture on the composition and analysis of coal and oil gases, this ingenious chemist shows, that the light produced by these, or by olefiant gas, even when concentrated so as to produce a sensible degree of heat, occasioned no change on the colour of muriate of silver, nor on a mixture of chlorine and hydrogen; while the light emitted by electrized charcoal, speedily affects the muriate, causes these gases to unite rapidly, and sometimes with explosion. The concentrated light of the moon, like that of the gases, produced no change. He concludes with stating, that he found the photometer of Mr. Leslie ineffectual. He employed one filled with the vapour of ether (renewable from a column of that fluid), which he found to be more delicate.

The general facts, says Sir H. Davy, of the refraction and effects of the solar beam, offer an analogy to the agencies of electricity. In the voltaic circuit, the maximum of heat seems to be at the positive pole, where the power of combining with oxygen

is given to bodies, and the agency of rendering bodies inflammable is exerted at the opposite surface; and similar chemical effects are produced by negative electricity, and by the most refrangible rays of the solar beam. In general, in nature, the effects of the solar rays are very compounded. Healthy vegetation depends upon the presence of the solar beams, or of light; and whilst the heat gives fluidity and mobility to the vegetable juices, chemical effects likewise are occasioned, oxygen is separated from them, and inflammable compounds formed. Plants deprived of light become white, and contain an excess of saccharine and aqueous particles; and flowers owe the variety of their hues to the influence of the solar beams. Even animals require the presence of the rays of the sun, and their colours seem materially to depend upon the chemical influence of these rays: a comparison between the polar and tropical animals, and between the parts of their bodies exposed, and those not exposed to light, shows the correctness of this opinion.

[To be continued.]

LXXIV. *Observations on certain luminous Meteors called Falling Stars. By Dr.* T. Forster, *M.B. F.L.S. &c.*

Hartwell, E. Grinstead, June 2, 1821.

Sir, — I observed this morning the Queries of Mr. J. Farey Sen., respecting Shooting Stars, as they are commonly called; and I beg leave through your useful Magazine, to communicate such information and references to this subject as at present occur to me. I have for many years been an attentive observer of these curious meteors, and have noticed their peculiarities, and the particular states of the atmosphere in which they occur, with a view to ascertain their nature, and their relation to electrical phænomena.

On the large meteors, such, for example, as the great meteor of August 18, 1783, many treatises have been written, to which I have referred in a small work on atmospheric phænomena*. I have there alluded to the probable connexion between these phænomena, and the aërolites called Meteoric Stones, and to the curious observations of the late M. J. A. De Luc on this subject.

How far the smaller meteors called Falling Stars may be referable to similar causes, I am at present unable to say; neither would I venture, in the present stage of the inquiry, to adopt any particular hypothesis to explain them. The first object in meteorology, as in all sciences, must be to obtain an accurate series of facts well arranged. With this view, I have for upwards of

* Researches about atmospheric Phænomena, by T. Forster, London, 1814, 2d edition, p. 114.

thirteen

thirteen years kept a journal of atmospherical phænomena, and at the same time have collected from the ancient writings of the Greek and Roman philosophers, whatever I could find that might bear on the object of my researches. For, disregarding all theories, the extreme accuracy of the observations of the Greek meteorologists renders them worthy of being referred to.

My own observations on falling stars have enabled me to divide them into three principal sorts. Of these three sorts, the largest are very brilliant and give considerable light: they move in curved as well as in straight lines, generally sloping downwards, and seldom very rapidly. The colour of their light is bright white, yellowish, or blueish, and very rarely copper coloured. They occur mostly on fine warm summer evenings when waneclouds abound; they seem to leave no train behind in general; when they do, it is under circumstances of peculiarity of atmosphere.

The next sort are much smaller, and occur in clear cold and frosty nights, both in winter and summer; and also in warm weather when east winds prevail. They shoot along with great rapidity, and leave no train, except such as I can refer to an effect produced on the retina of the eye by the rapid passage of the light;—such a *deceptio visus*, for example, as Homer ascribes to the δολιχοσκιον εγχος.

The third sort of meteors are small in general like the last, but are distinguished by leaving a long train of light behind them, which lasts some seconds after the star is extinguished. This kind almost always forebode windy weather; and when they occur, I have noticed that all meteors which happen on the same evening leave the aforesaid long white tails behind them. These long tracts of white light are accurately noticed by Virgil in *Geor.* lib. i.

> "Sæpe etiam stellas, vento impendente, videbis
> Præcipites cœlo labi, noctisque per umbram
> Flammarum longos a tergo albescere tractus."

But Aratus the astronomical poet has given a still more beautiful and accurate description of them. The following passage occurs somewhere in the Diosemea:

> *Και δια νυκτα μελαιναν ὁτ' αςερες αἴσσωσσαι*
> *Ταρφεα τοι δ' οπθεν μυροι ὑπολευκαινωνται,*
> *Δειδελθαι κεινοις αυτην ὁδον ερχομενοιο*
> *Πνευματος.*

Very copious observations on their appearance and indications may also be found in Plin. H. N. xviii. 35.—Aristot. Meteor. lib. i. c. 4.—Lucret. ii. 206, and v. 1190.—Seneca Nat. Quæst. i. 14. Refer also to Bertholon. Elect. Met. Lyons, 1784, and

 to

to that immense code of meteorology collected from all parts of Europe, and published in five quarto volumes by the Society of Meteorology formed and directed by the late Elector Palatine.

I have the honour to remain, sir, &c.

T. Forster.

To the Editor of the Phil. Mag.

LXXV. *Appendix to the Third Report of the Commissioners appointed by His Majesty to consider the Subject of Weights and Measures.*

The Commissioners having been furnished, by the kindness of the Hon. Charles C. C. Jenkinson, with the apparatus employed by the late Sir George Shuckburgh Evelyn, in the determination of the magnitude of the standard weights, and there being some doubt of the perfect accuracy of his method of measuring the capacity of the bodies employed, it was judged necessary to repeat that measurement with greater precautions; and the results of Captain Kater's experiments have afforded some slight corrections of the capacities in question.

The sides of Sir George Shuckburgh's cube were found by Captain Kater equal to 4·98911, 4·98934, and 4·98935 inches; the diameter of the cylinder 3·99713, and its length 5·99600 inches; and the diameter of the sphere 6·00759 inches. Hence the content of the cube appears to be 124·1969 inches; that of the cylinder 75·2398; and that of the sphere 113·5264 inches of Bird's Parliamentary Standard of 1760, recommended in the last Report of the Commissioners, or of the Standard made by Troughton for Sir George Shuckburgh.

The difference of the weight of the cube in air at 62°, with the barometer at 29·0, and in water at 60·2°, was 31381·79 grains; and, adding to this the weight of an equal bulk of the air at 62°, which is $\frac{1}{834}\cdot\frac{29}{30}$ of that of the water, or 36·26 grains, and subtracting from it $\frac{1}{8\cdot5}$ of this, or 4·26 grains, the buoyancy of the brass weights, we obtain 31413·79 grains for the weight of the cube of water in a vacuum at 60·2°. Now this cube is less than the supposed measure at the standard temperature of 62°, in the ratio of 1 to 1·0000567, on account of the contraction of the brass; and the water is denser than at the standard temperature, according to Mr. Gilpin's experiments, in the ratio of ·99998 to ·99981, or of 1·00017 to 1, the whole correction, for the difference of 1·8°, being ·0001133, or 3·55 grains, making 31410·24 for the weight of the cube of water in a vacuum at 62°; which, divided by 124·1969, gives 252·907 for the weight of a cubic inch in Sir George Shuckburgh's grains.

In

In the same manner, we obtain for the cylinder, which was weighed in air under the same circumstances, and in water at 60·5°, the difference being 19006·83 grains, the correction $\frac{1}{834}\cdot\frac{29}{30}\cdot\frac{7\cdot5}{8\cdot5}$ for the effect of buoyancy, amounting to 19·43 grains; and for the difference of temperature of the water and brass conjointly, the densities being ·999955 and ·999810, the correction ·000145 — ·000047 = ·000095, or 1·80 grains, leaving + 17·63 grains for the whole correction of the weight, as reduced to a vacuum at 62°, and making it 19024·46, which divided by 75·2398, the content of the cylinder, affords us 252·851 for the cubic inch in a vacuum at 62°.

The sphere was weighed in air at 67°, the barometer standing at 29·74; the correction for buoyancy is here $\frac{7\cdot5}{8\cdot5}\cdot\frac{29\cdot79}{30}\cdot\frac{1}{843}$, or for 28673·51 grains, 29·72; while the temperature of 66° requires, for the difference between the expansion of brass and water, the addition of ·00042 — ·000126, or ·000294 of the whole, that is +8·43 grains, making the whole correction 38·15, and the weight in a vacuum 28711·66; which, divided by 113·5264, gives us 252·907, for the cubic inch in a vacuum.

The mean of these three measures is 252·888, giving for the three errors +·019, —·037, and +·019; and this mean, reduced to the Parliamentary Standard, makes 252·722 grains, for the cubic inch of distilled water at 62°, weighed in a vacuum, or 252·456 in air, under the common circumstances of the atmosphere, when weights of brass are employed. In a vacuum at the maximum of density, that is at 39°, the weight of a true cubic inch will be 253 grains, and of a cubic decimetre 15440 *. The proposed Imperial gallon, of ten pounds, or 70000 grains, of water, will contain very nearly 277·3 cubic inches, under common circumstances.

LXXVI. *Observations on the Letter in last Number respecting the Calculation of the Depression of Mercury in Capillary Tubes* †. *By* James Ivory, *M.A. F.R.S.*

Sir, — I find in your last publication, another letter on the subject of the depression of mercury in capillary tubes, full of wit, and banter, and heavy charges of error, but containing nothing new in point of argument.

In the *Conn. des Tems* 1812, Laplace published an article on the Depression in Barometer-tubes, accompanied with a Table. The illustrious foreigner notices a similar Table, computed by

[* It appears, however, from an official Report, obligingly communicated to us by Dr. Kelly, that the actual standard chilogramme has been found to contain only 15433 English grains.]

† See Phil. Mag. for last month, p. 376.

means

means of the *Series*, and published in England in Nicholson's Journal 1809. He objects to the English mode of computing, on the score of inaccuracy and the length of the calculations; and he therefore prefers another method, which is sneered at in the article COHESION in the Supplement of the *Encyclopædia Britannica*. A translation of Laplace's article was immediately published in England in the same Journal 1810. The translator makes no observations in words; but certain passages to be animadverted on are distinguished by placing marks of admiration; the degrees of disapprobation being noted, in this symbolical criticism, by multiplying the signs. At the end of the translation, the English and French numbers are set against one another in hostile array: as much as to say: Here are the numbers of the infallible *Series*; and see there, the numbers of the blundering Frenchmen with all their refined methods of calculation.

It was certainly to be expected that a grave philosopher, such I must suppose my antagonist to be, would take proper pains, before uttering injurious aspersions, to ascertain whether the charges he was sending forth into the world were just or not. Supposing him to have confidence in his series, he had in his own power a sure way of verifying any suspicions of error he might entertain. He had only to take the elements from which I computed, or the elements from which the French computed; to substitute them in his own formula; and then to compare the results obtained with the results of the other methods. I see no reason to think he has taken this precaution. The burden of his song is nothing but this: My numbers are different from yours, and therefore yours must be wrong.

I certainly have a great aversion to meddle with the clumsy series of my antagonist; but I must now take it in my own hands, and do with it what he ought to have done. He has in fact left me no other mode of defence; for, if I pursue any other line of argument, I shall only be told that I have committed new errors. It were perhaps more prudent to trust the characters of the methods to the opinion of the public; but still it may be worth while taking some pains to oppose so overbearing an attempt, even although the chance be small that the truth shall prevail against the weight of authority.

Besides the letters in the series in the article COHESION in the Supplement of the *Encyc. Brit.*, I shall put $y = b\,x$, and $a =$ the depression: then $a = \frac{2.y}{q\,x}$. According to the elements I use, $q = 196 = 14^2$, and $s = \cdot 735$.

First, let $2x = 0\cdot 5$; then $qx^2 = 12\cdot 25$; which number being substituted in the series, we get this equation,

$$\cdot 735 = 3\cdot 546\,y + 3\cdot 019\,y^3 + 6\cdot 695\,y^5.$$

For

For the coefficients of this equation, 5 terms of the first series were used; 7 terms of the second; and 5 of the third; but as all these last terms were diverging, I reckon the coefficient about half the true value. Means are afforded of forming a nearer estimate; but, as the cost would outgo the profit, I shall decline the labour of the calculation. We have next an equation to solve. Truly, the *Series* ought to have for its peculiar motto, *Labor omnia vincit improbus.*

The solution of the equation is, $y = 0{\cdot}1997$;

and the proof .. .. ·7083

247

21

·7351

Now, $a = \frac{2y}{49} = {\cdot}00815$, the same number found by both my rules.

The number ·1997 is no doubt a little too great, on account of the omissions: it may be ·1996, or ·1995, or even less; but this is not material.

Next, let $2x = {\cdot}6$; then $qx^2 = 17{\cdot}64$; and we get this equation, $${\cdot}735 = 5{\cdot}574\,y + 12{\cdot}07\,y^3.$$

No fewer than 7 terms of the first series and 9 of the second were used to find the two coefficients of this equation, of which the last is only true in the integers. In the next coefficient-series, the last term set down, or the tenth, is a whole number of two figures; so that we could only have a rough estimate of the coefficient of y^5. I shall decline the calculation, by which the uncertainty of the approximation would not be removed, although it might be a little diminished. The solution is,

$y = {\cdot}1274$; and the proof .. .. ·7101

249

·735

The number $0{\cdot}1274$ is too great; but I make a large allowance when I say that the true value of y is greater than $0{\cdot}1264$: and if both the numbers be substituted in the formula, $a = \frac{2y}{qx}$, we get, for the depression ·00433 and ·00429, between which the truth certainly lies. Now my number ·00431 is between these limits; and the series itself, with fair computation, has vindicated my rules in this instance also, against the charges of its inventor. I observe that my antagonist neglects the just grounds of calculation, and amuses himself with making conjectural additions to another series different from the first. He ought to recollect, that the accuracy of the conclusion will depend entirely upon the exactness of the first series.

My

My antagonist has contended stoutly for the accuracy of ·00416, the depression in a bore of 0·6. Let us apply to this instance, the test of my rules. As we both use the ultimate product ·015 for one element, and differ only in the sine of depression, if we substitute his number ·75 for z in computing the limit of f in the second rule in the article FLUIDS, we shall get

$$\therefore \quad a = \frac{\cdot 01443}{2x\cdot\lambda},$$

using the same letters as above. Now in the article COHESION, I find $\lambda = 5\cdot 737$; and hence $a = \cdot 00419$, very little different from his number, which he admits may be carried to ·00418. And if he will compute veraciously, he will find that ·00419 is the most probable number; for his method cannot reach beyond probability.

On the whole, his only argument has no foundation. My number is not wrong, because it is different from his. In the only instance he has ventured to adduce, the difference of the two numbers arises from the difference of the elements of calculation; since these elements being substituted in the same formula bring out the two depressions. But if ·00416 be the true depression with the elements ·015 and ·75, it can hardly be right in the Table of 1809, with the elements ·015 and ·7353.

But the inaccuracy and deficiency of the series, which have already appeared in the foregoing examples, are most conspicuous in the middle of the Table between the bores 0·2 and 0·4. As an example, let us take the bore 0·3 in the Table 1809, the elements being ·015 and ·7353. In this case $q = \frac{10000}{51}$, and $qx^2 = 4\cdot 41176$.

Now, by the formula in your last Number, $t = \frac{qx^2}{4} = 1\cdot 10294$; and we get,

	$-\ \cdot 000805$
	$-\ \quad\ \ 85$
	$-\ \quad\ \ 12$
Sum of negative terms	$-\ \cdot 000902$
Positive terms $= \frac{\cdot 015}{2x.\lambda} =$	$+\ \cdot 030077$
Depression	$\cdot 029175$

Applying to the same example the first rule in the article FLUIDS, I have found $f = 1 - \cdot 0301$; and, λ being $1\cdot 6627$, we get the depression $= \frac{\cdot 015}{2x.\lambda} \times f = \cdot 030077 \times (1 - \cdot 0301) = \cdot 029171$.

The two rules therefore agree; but as they are out of the pale, their authority is zero. We must have recourse to the series.

By

By making $qx^2 = 4{\cdot}41176$, we get this equation,

$$\cdot 7353 = 1{\cdot}6627\, y + 0{\cdot}2285\, y^3 + 0{\cdot}215\, y^5 + 0{\cdot}149\, y^7.$$

In computing these coefficients, 5 terms of the first series were used; 6 terms of the second; 5 of the third; and all the terms set down of the fourth. The solution of the equation is,

$$y = 0{\cdot}4292; \text{ the proof, } \quad .. \quad \begin{array}{r} \cdot 71363 \\ 1807 \\ 313 \\ 40 \\ \hline \cdot 73523 \end{array}$$

We now get the depression $= \cdot 068 \times y = \cdot 029185$, very near the former values; and the difference is on the right side, y being too great. Now the number in the Table is ·02906; and no reasonable man can doubt that there is an error.

I have now refuted the arguments, or surmises rather, of my antagonist; and have shown him that his series is not infallible. My rules have been laboriously vindicated with the fingers at least, if not with the head. As to the so vaunted method of computation by the series, there can be but one opinion. It is inelegant; operose; unsatisfactory and unscientific, since it affords no means of ascertaining the degree of the approximation but sheer calculation, which in many cases is impracticable.

We hear much of refinements in the mathematics. Does this allude to the results of calculation? Now my rules are greatly more simple and easier in practice than his, which can hardly be a fault. It must then allude to the process of investigation. But I have always understood that pains must be taken to search into the properties of the quantities under consideration, in order to obtain simple and effectual rules.

It is proposed to combine the series with Laplace's method of approximation. The attempt will be successful, if it be executed with judgement and patient labour. But is it any thing new to assist an approximation by the method of development? The density of the air, considered as a function of the refraction it causes, has been expanded in a series; and by this means, with some helps, we have got a new rule for calculating the refraction of the atmosphere: but we have obtained nothing new in point of mathematical method. New applications do not make new methods; and new names do not constitute new things. The theorem of Taylor, or of McLaurin, has received no additions, even although the mode should prevail of designating such well-known operations by the quaint phrase of the *Universal Solvent*, or the sonorous epithet of the *Taylorian Theorem*.

I have the honour to be, &c.

June 11, 1821. J. Ivory.

P. S. An imperfect method and exceptionable calculations are

bad grounds on which to found charges of error. Before this dispute be carried further, it is recommended that the numbers in the Table of 1809 be computed anew by just rules: then, if attention be paid to the difference of the elementary quantities and to the degree of accuracy aimed at, greater progress will be made in clearing up this question, than would be done by whole volumes of loose discussion. J. I.

LXXVII. *Notice respecting the real Inventor of the Steam-Engine.*

THE following communication by R. N. (*i. e.* as I conceive, R. Naires) which appeared in the Gentleman's Magazine in the year 1811, deserves to be again brought before the public, that the merit of the invention may, as it ought, be ascribed to its true author.—A. T.

July 3, 1811.

Mr. URBAN,—The inventors of valuable improvements are generally thought worthy of celebration, and their names are sometimes sought with eagerness for the sake of doing justice to their merits. To such distinction few inventors seem more amply entitled than the person to whom we owe the Steam-engine; a contrivance which, assisted by modern improvements, is now performing what, a century ago, would have seemed miraculous or impossible; yet it appears that he has been hitherto entirely unknown to the world at large.

In 1699, a Captain Savary obtained a patent for this invention; and he has consequently occupied all the honour of the discovery. But in that noble assemblage of MSS. the Harleian Collection, now in the British Museum, the strongest testimony appears that the real inventor was Samuel Morland, who was Master of the Works to Charles II. and who I fancy was knighted; for in the MS. he is called Sir Samuel, and "*Le Chevalier.*" That the first hint of the kind was thrown out by the Marquis of Worcester, in his Century of Inventions, is allowed; but obscurely, like the rest of his hints. But Morland wrote a book upon the subject; in which he not only showed the practicability of the plan, but went so far as to calculate the power of different cylinders. This book is now extant in manuscript in the above collection. It was presented to the French King in 1683, at which time experiments were actually shown at St. German's. The author dates his invention in 1682; consequently 17 years prior to Savary's patent. As Morland held places under Charles II., we must naturally conclude, that he would not have gone over to France to offer his invention to Louis XIV. had he not found

it slighted at home. It seems to have remained obscure in both countries till 1699, when Savary, who probably knew more of Morland's invention than he owned, obtained a patent; and in the very same year, M. Amontons proposed something similar to the French Academy, I believe, as his own.

The description of the manuscript in which Morland explains his invention will be found in the improved Harleian Catalogue, vol. iii. No. 5771; and it is also pointed out in the preface to that volume, sect. xxii; but hitherto seems to have been as little noticed as Morland himself. But if he was the real inventor, as these circumstances seem to render certain, it is highly necessary that his name should in future be recorded with all the honour which an invention of such utility demands.

I shall just add, respecting the same catalogue, that the Biblical collections in No. 7522 were made by *Patricius Junius,* that is, Patrick Young, who, as King's librarian, had the care of the Alexandrine MS., and had thoughts of publishing it. This is nearly proved by Woide, in the preface to his edition of the N. T. sect. 16, but was not recollected when that article was written.

Yours, R. N.

LXXVIII. *On the Efficacy of Yeast, and the Application of Vinegar in Putrid Fever.* *By* ROBERT JOHN THORNTON, *M.D.*

Letter from the late Earl of EXETER.

DEAR DOCTOR,—I AM sure you will be truly sorry to hear what a dismal house we have had since our return home. My dear son Cecil was taken ill the day after we got home, which was on the 3d of last month. It turned out to be a putrid fever. He was attended night and day by Dr. John Willis, and occasionally by a Stamford physician, Dr. Arnold, and by Lady Exeter constantly. He became so bad at last, that the Doctors said, they could do no more for him. All irritability of the eye-sight was gone, no passage down his throat could be effected, and a rattling in the throat indicated the last moments to be arrived. Our poor dear Lady Exeter and Dr. John were the miserable bearers of this dreadful news to me. Having recollected your account of the good effects of vinegar in the case of the Rev. Mr. Townsend, and others; I wished this to be tried. His body was bathed with the vinegar, he took yeast inwardly, and it was applied in clysters; and in twelve hours he was so far amended, as to raise in us all hopes, and in four days he was convalescent, and is now lively and jocose.

I cannot conclude this letter without relating a discovery of Dr. John Willis, which ought to be generally known. In cases of

 spitting

spitting of blood he never fails to give a vomit, and it always stops the effusion of blood. He tells me, it has never failed him. Wishing you every success in your laudable endeavours to relieve the distresses of others, and extend the science of medicine,

I remain, dear Doctor,

Yours very sincerely,

EXETER.

Case of Mortification cured by the Application of Yeast outwardly, and by the Form of Injection inwardly.*

Glasgow, June 7, 1816.

DEAR SIR,—My son Robert (now a man) when a child about two years old, had the chicken pox, after which, in consequence of catching cold, in the course of a single night a mortification took place across his belly; on the outside it had the appearance of a mazarine blue ribband laid across from the one side to the other. Immediately cloths dipt in barm or yeast were applied, repeating them as often as they began to dry, at the same time giving him injections of barm; they were given cold, to prevent them being thrown off too soon. The reason of giving him the injections was, that he would not swallow the barm; and the Doctor assured me, the whole mass of blood was in a state of corruption; and indeed it appeared so, for in a few days after the first appearance of mortification there arose on one of his temples a gathering about the size of a pigeon's egg, of a very dark colour, and when it broke it discharged a black watery kind of stuff: before the first broke, a second began to gather on the other temple; it was longer in coming to maturity, but when it did break it discharged a good thick yellow matter, when the Doctor pronounced him out of danger. After the application of the barm, the blue part across the belly turned of a yellow colour, which the Doctor took out, and then the wound had the appearance of a mouth wide open; it could easily receive the side of a hand laid in. The barm was continued for ten days or a fortnight, till every appearance of danger was over; after that, there was some kind of simple ointment applied to heal up the wound, which now has the appearance of a deep burn after being healed.

His diet during his illness consisted chiefly of fruit.

To Mr. Tilloch. JANET ALLAN."

* The last of these cases I communicated to Dr. Thornton, who thinks it deserves to be made generally known. The writer is a relation of my own. When R. Allan was taken ill, an able medical gentleman of Glasgow was instantly sent for. He frankly told the parents that there was no possibility of the child recovering. It was in consequence of this; and of Mrs. Allan having read, in one of the early volumes of the Philosophical Magazine, of the curative powers of yeast in putrid cases, that this remedy, and with such happy success, was resorted to.—A. T.

LXXIX. *A*

LXXIX. *A Communication relative to a Correspondence between Dr.* Henry *and Dr.* Ure.

To the Editor of the Philosophical Magazine.

Sir, — In page xiii of the Introduction to the Dictionary of Chemistry lately published, I have alluded to Dr. Henry in terms which have occasioned a private correspondence between that gentleman and me, the result of which we are desirous of making public in your Journal.

In the beginning of August 1816, I transmitted to him an Essay on Alkalimetry and Acidimetry, accompanied by a letter, in which I begged him to favour me with his opinion of its merits, cautioning him, meanwhile not to communicate its contents to any person. In the 8th edition of his Elements, which appeared in 1818, he published a plan of alkalimetry and acidimetry modified from that described in my Essay*. This struck me at the time as an unwarranted use of my communication; and declining to correspond with him on the subject, I resolved to seize the first favourable opportunity to reclaim my rights. Under this feeling I wrote the paragraph in the Introduction to the Dictionary.

Dr. Henry thus writes me on the 12th of April 1821, "I assure you that I had not at the time of publishing my book, nor can I now recall, the remembrance of any injunction of secrecy, respecting your alkalimeter; I conceived I had so expressed myself at page 512, vol. ii. of my Elements, as unequivocally to give to you the credit of inventing an instrument on the principle of directly, and without calculation, indicating the *per centage* of alkali in any specimen; and that I pretend to nothing more than the modification of your method which is described in my book."

Under these circumstances, I am satisfied that Dr. Henry had no intention to appropriate to himself the credit of my invention; but I sincerely regret that, before promulgating the modification of my method, he had not consulted me on the subject. This would have prevented all chance of misunderstanding between me and Dr. Henry, whose accomplishments as a gentleman and a chemist, I have been accustomed to admire. The readers of the Dictionary will perceive under the articles Calculi, Coal-

* "It has been very properly objected to it [the alkalimeter of Descroisilles] by Dr. Ure of Glasgow, (in an Essay on Alkalimetry, which he was so good, about two years ago, as to communicate to me in manuscript, and which I believe he has not yet published,) that these degrees, being entirely arbitrary, do not denote the value of alkalis in language universally intelligible; and he has proposed an instrument which shall at once, and without calculation, declare the true proportion of alkali in 100 parts of any specimen. The principal deviation in the following rules from the method of Dr. Ure, is," &c. &c.

GAS,

GAS, GAS, SALT, &c. that I have not suffered temper to influence my judgement, but have done merited honour to the Doctor's researches on every scientific occasion.

I have the honour to be, sir,
Your most obedient servant,

Glasgow, April 15, 1821. ANDREW URE.

LXXX. *Report from the Select Committee of the House of Commons, on Weights and Measures. With an Appendix.*

THE Select Committee appointed to consider of the several Reports which have been laid before this House, relating to Weights and Measures, and of the Proceedings which have taken place for determining the Length of the Pendulum vibrating Seconds, and to report their Observations and Opinion thereupon to the House;—Have considered the matters referred to them, and have agreed to the following REPORT:

YOUR Committee concur entirely in opinion with the Commissioners on Weights and Measures, as to the inexpediency of changing any standard, either of length, superficies, capacity, or of weight, which already exists in a state of acknowledged accuracy; and where discrepancies are found between models equally authentic, they deem it right that such a selection should be made as will prove most accordant with generally received usage, and with such analogies as may connect the different quantities in the most simple ratios.

They also concur in recommending, that the subdivisions of weights and measures employed in this country be retained, as being far better adapted to common practical purposes than the decimal scale.

For the reasons assigned by the Commissioners, your Committee recommend that the Parliamentary brass standard of three feet, now in the possession of the House of Commons, and made by Bird in 1760, be henceforth considered as the authentic legal Standard of Length of the British Empire, so that the distance besween the centres of the two gold pins inserted in that scale, the brass being at the temperature of sixty-two degrees by Fahrenheit's thermometer, be one yard: And it appears from the experiments made for determining the length of the Pendulum vibrating seconds at London in a vacuum, and reduced to the level of the sea, that the distance from the axis of suspension to the centre of oscillation of such a Pendulum, is 39·1393 inches of the above standard distance: and that the length of a platina metre at the temperature of thirty-two degrees of Fahrenheit's thermometer, supposed to be the ten-millionth part of the quadrant

drant of the meridian, corresponds with 39·3708 inches of the said distance.

Your Committee recommend, that superficial measures remain as they are now defined by law, namely, that the perch, pole or rod, be a square of $16\frac{1}{2}$ feet, that the acre consists of 160 such perches, and so of the rest.

They further recommend, that the standard brass weight of two pounds, also in the possession of the House of Commons, and made in 1758, be considered as authentic; that one half thereof as gravitating in air at the mean height of the barometer and with the thermometer at 62°, be henceforth the legal Troy Pound of the British Empire, containing 5760 grains; and that 7000 grains troy be declared to constitute a pound avoirdupois.

And it appears that a cubic inch of distilled water weighs in a vacuum, opposed to brass weights in a vacuum also, at the temperature of 62 degrees of Fahrenheit's thermometer, 252·72 such grains; and consequently a cubic foot of distilled water, under similar circumstances, will weigh 62·386 pounds Avoirdupois.

In proceeding to measures of capacity, which, for convenience, your Committee have postponed to those of weight, they find themselves embarrassed, as the Commissioners have been, not only by various measures designated by the same name, but by a discrepance in the multiples and sub-multiples of the same measure. They are on the whole, however, induced to believe, that the gallon of England was originally identical for all uses; and that the variations have arisen in some cases from accident, and in others from fraud.

The definition of a Winchester bushel, in the Act of King William, for laying a duty on malt, seems to have been made for the purpose of facilitating the construction of cylindrical measures by a near coincidence, without minute fractions. From this definition, the dry gallon would consist of 268·835 cubic inches.

The Gallon measure in the Exchequer contains 270·4 cubic inches; and derived from the pint, quart, &c. the gallon will stand as follows:

From the bushel	266·1 cubic inch.
From the definition by King William	268·8
From the gallon measure	270·4
From the pint	276·9
From the quart	279·3
By an Act of Parliament made for revenue purposes, the beer gallon	282·
By an Act 42 Geo. III. the Winchester gallon is estimated at	$272\frac{1}{4}$

The Wine Gallon is supposed to have continued gradually shrinking

shrinking in dimensions, till its progress was arrested by a fiscal definition at 231 cubic inches.

This last measure differs so materially from all the rest, that it must either be retained as one quite distinct, and applicable to its peculiar uses, or, as seems most expedient, it must be abolished. But, amidst the variations and uncertainty of the remainder, your Committee agree with the Commissioners, in recommending that they may be all brought back to an equality, and at the same time made to bear a simple relation to the standard of weight, by taking the pint for a basis, which contains 20 ounces of distilled water avoirdupois, at the temperature of 62°, as nearly as it is possible to ascertain by experiment, on a vessel of that construction and workmanship.

If then the pint be considered as equal in bulk to 20 ounces of distilled water, at the temperature of 62°, the cubic inch weighing 252·456 grains in air, at the mean height of the barometer, the imperial gallon will contain 277·276 cubic inches, weighing exactly ten pounds.

If the proposition now submitted should be sanctioned by the House, your Committee recommend that leave be given to bring in a Bill for declaring these Standards of Length, of Capacity, and of Weight, to be the imperial Standards for Great Britain and Ireland, and for its colonies and dependencies; and they recommend that several copies of the standards be made with the utmost possible accuracy for the use of the Exchequer, for the three capitals, for the principal foreign possessions, for the Government of France, in return for the communication of their standards; and especially for the United States of America, where your Committee have reason to believe that they will be adopted, and thus tend, in no small degree, to facilitate the commercial intercourse, and by so doing to consolidate a lasting friendship between the two great nations of the world most assimilated by their language, their laws, religion, customs and manners.

Your Committee cannot close their Report, without adverting to the extraordinary knowledge and ingenuity, and to the indefatigable industry displayed by Captain Kater, by whom all the experiments have been gratuitously conducted, for ascertaining the various standards, and for determining the length of the Pendulum by a method peculiarly his own, and by which he has arrived at a degree of accuracy and precision, that, but a few years since, was declared to be utterly unattainable.

This gentleman, in compliance with His Majesty's directions, given in pursuance of an address of this House, has also observed the variations of the Pendulum on the principal stations of the Trigonometrical Survey; and from these observations, deductions have

have been made of great importance with respect to the general figure of the earth, its density and internal construction. So that your Committee are decidedly of opinion, that it will be highly proper to extend similar observations over a still larger surface, so as to connect the measurements and astronomical observations made by the different nations of Europe, as much as possible, into one whole.

Your Committee having directed their attention to the best and most practicable method of bringing the imperial measures into general use, beg leave further to recommend a legislative enactment, by which it shall be declared, that all bargains and sales, where nothing appears to the contrary, shall be deemed and taken to be made in conformity with these measures of length, superficies, capacity, and weight; but that for a time to be limited, it shall be competent for all persons to deal by any other measures, established either by local custom, or founded on special agreement, that they may select; provided always, that the ratio or proportion of such local measures, to those established by law, may be a matter of common notoriety; and that in the case of a special agreement, the ratio or proportion be therein expressed.

Your Committee subjoin in an Appendix, some computations and proportions, which they think may be of general use.

28 May 1821.

Appendix.

The Pendulum vibrating seconds of mean solar time at London in a vacuum, and reduced to the level of the sea, 39·1393 inches, consequently the descent of a heavy body from rest in one second of time in a vacuum, will be 193·145 inches. The logarithm 2·2858828.

A platina metre at the temperature of 32°, supposed to be the ten-millionth part of the quadrant of the meridian, 39·3708 inches. The ratio to the imperial measure of three feet as 1·09363 to 1, the logarithm 0·0388717.

The five following standards have been measured, as follows:

Gen. Lambton's scale, used in the Trigonometrical Survey of India	35·99934 inches.
Sir George Shuckburgh's scale (which for all purposes may be considered as identical with the imperial standard)	35·99998
Gen. Roy's scale	36·00088
Royal Society standard	36·00135
Ramsden's bar	36·00249

Weight of a cubic inch of distilled water in a vacuum at the temp. 62°, as opposed to weights in a vacuum also, 252·722 grains	l. 2·4026430
Consequently a cubic foot 62·3862 p. avoirdupois	l. 1·7950887
Weight of a cubic inch of distilled water in air at 62° of temperature with a mean height of the barometer .. 252·456 gs	l. 2·4021857
Consequently a cubic foot 62·3206 p. avoir.	l. 1·7946314
And an ounce of water 1·73298 cubic inches.	l. 0·2387924
Cubic inches in the imperial gallon 277·276	l. 2·4429124
Diameter of the cylinder containing a gallon at one inch high .. 18·78933	l. 1·2739112

Specific gravity of water at different temperatures, that at 62° being taken as unity.

°		°		°	
70	0·99913	56	1·00050	44	1·00107
68	0·99936	54	1·00064	42	1·00111
66	0·99958	52	1·00076	40	1·00113
64	0·99980	50	1·00087	38	1·00113
62	1·	48	1·00095		
58	1·00035	46	1·00102		

The difference of temperatures between 62° and 39°, where water attains its greatest density, will vary the bulk of a gallon of water, rather less than the third of a cubic inch.

And assuming from the mean of numerous estimates the expansion of brass 0·00001044 for each degree of Fahrenheit's thermometer, the difference of temperatures from 62° to 39° will vary the content of a brass gallon measure just one-fifth of a cubic inch.

It appears that the specific gravity of clear water from the Thames, exceeds that of distilled water at the mean temperature, in the proportion of 1·0006 to 1, making a difference of about one-sixth of a cubic inch, on a gallon.

Rain water does not differ from distilled water, so as to require any allowance for common purposes.

LXXXI. *A Table of the Reduction of the Ecliptic to the Equator to every Ten Minutes of the Longitude of the Points of the Ecliptic. With the Differences, and Variation of the Reduction, for Ten Seconds Variation of the Obliquity, of the Ecliptic, for Jan.* 1, 1801. (*Obliquity Ecliptic* 23° 27′ 57″.)

Argu-

Argument.	Signs 0.—— and VI.——			Signs I —— and VII ——			Signs II.—— and VIII ——			Argument.
Long. of the Eclip.	Reduction.	Diff.	Var. 10″ Obl.	Reduction.	Diff.	Var. 10″ Obl.	Reduction.	Diff.	Var. 10″ Obl.	Long. of the Eclip.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
0 0	0 0 0·00	49·62	0·00	2 5 39·16	26·78	1·80	2 11 11·24	24·81	1·95	30 0
10	0 0 49·62	49·62	0·01	2 6 5·94	26·55	1·80	2 10 46·43	25·11	1·95	50
20	0 1 39·24	49·62	0·02	2 6 32·49	26·31	1·81	2 10 21·32	25·38	1·94	40
30	0 2 28·86	49·61	0·03	2 6 58·80	26·06	1·81	2 9 55·94	25·64	1·94	30
40	0 3 18·47	49·61	0·04	2 7 24·86	25·82	1·82	2 9 30·30	25·94	1·93	20
50	0 4 8·08	49·60	0·05	2 7 50·68	25·60	1·82	2 9 4·36	26·21	1·93	10
1 0	0 4 57·68	49·59	0·07	2 8 16·28	25·32	1·83	2 8 38·15	26·48	1·92	29 0
10	0 5 47·27	49·57	0·08	2 8 41·60	25·08	1·84	2 8 11·67	26·77	1·92	50
20	0 6 36·84	49·57	0·09	2 9 6·68	24·85	1·84	2 7 44·90	27·07	1·91	40
30	0 7 26·41	49·56	0·10	2 9 31·53	24·59	1·85	2 7 17·83	27·31	1·90	30
40	0 8 15·97	49·54	0·11	2 9 56·12	24·34	1·85	2 6 50·52	27·60	1·90	20
50	0 9 5·51	49·52	0·12	2 10 20·46	24·11	1·86	2 6 22·92	27·86	1·89	10
2 0	0 9 55·03	49·51	0·13	2 10 44·57	23·84	1·87	2 5 55·06	28.16	1·88	28 0
10	0 10 44·54	49·49	0·14	2 11 8·41	23·59	1·88	2 5 26·90	28.41	1·88	50
20	0 11 34·03	49·46	0·15	2 11 32·00	23·35	1·89	2 4 58·49	28.69	1·87	40
30	0 12 23·49	49·45	0·17	2 11 55·35	23·09	1·90	2 4 29·80	28.98	1·87	30
40	0 13 12·94	49·42	0·18	2 12 18·44	22·85	1·90	2 4 0·82	29.24	1·86	20
50	0 14 2·36	49·39	0·20	2 12 41·29	22·57	1·91	2 3 31·58	29·50	1·86	10
3 0	0 14 51·75	49·37	0·22	2 13 3·86	22·33	1·92	2 3 2·08	29·77	1·85	27 0
10	0 15 41·12	49·34	0·23	2 13 26·19	22·28	1·92	2 2 32·31	30·05	1·84	50
20	0 16 30·46	49·31	0·24	2 13 48·47	21·61	1·93	2 2 2·26	30·30	1·83	40
30	0 17 19·77	49·28	0·25	2 14 10·08	21·54	1·93	2 1 31·97	30·59	1·83	30
40	0 18 9·05	49·25	0·26	2 14 31·62	21·30	1·94	2 1 1·37	30·83	1·82	20
50	0 18 58·30	49·22	0·27	2 14 52·92	21·06	1·94	2 0 30·54	31·11	1·81	10
4 0	0 19 47·52	49·17	0·28	2 15 13·98	20·76	1·95	1 59 59·43	31·37	1·80	26 0
10	0 20 36·69	49·14	0·29	2 15 34·74	20·52	1·95	1 59 28·06	31·63	1·79	50
20	0 21 25·83	49·11	0·30	2 15 55·26	20·27	1·96	1 58 56·43	31·88	1·78	40
30	0 22 14·94	49·06	0·32	2 16 15·53	19·98	1·96	1 58 24·55	32·17	1·78	30
40	0 23 4·00	49·02	0·33	2 16 35·51	19·75	1·97	1 57 52·38	32·40	1·77	20
50	0 23 53·02	48·98	0·34	2 16 55·26	19·44	1·97	1 57 19·98	32·67	1·76	10
5 0	0 24 42·00	48·94	0·35	2 17 14·70	19·21	1·97	1 56 47·31	32·91	1·75	25 0
10	0 25 30·94	48·89	0·36	2 17 33·91	18·92	1·98	1 56 14·40	33·19	1·74	50
20	0 26 19·83	48·84	0·37	2 17 52·83	18·67	1·98	1 55 41·21	33·44	1·73	40
30	0 27 8·67	48·79	0·38	2 18 11·50	18·37	1·99	1 55 7·77	33·69	1·73	30
40	0 27 57·46	48·74	0·39	2 18 29·87	18·13	1·99	1 54 34·08	33·93	1·72	20
50	0 28 46·20	48·70	0·40	2 18 48·00	17·89	2·00	1 54 0·15	34·21	1·71	10
6 0	0 29 34·90	48·64	0·42	2 19 5·89	17·57	2·00	1 53 25·94	34·44	1·70	24 0
10	0 30 23·54	48·58	0·43	2 19 23·46	17·32	2·01	1 52 51·50	34·69	1·69	50
20	0 31 12·12	48·53	0·44	2 19 40·78	17·04	2·01	1 52 16·81	34·94	1·68	40
30	0 32 0·65	48·48	0·45	2 19 57·82	16·79	2·02	1 51 41·87	35·18	1·68	30
40	0 32 49·13	48·41	0·46	2 20 14·61	16·48	2·02	1 51 6·69	35·45	1·67	20
50	0 33 37·54	48·35	0·47	2 20 31·09	16·22	2·03	1 50 31·24	35·69	1·66	10
7 0	0 34 25·89	48·29	0·48	2 20 47·31	15·96	2·03	1 49 55·55	35·91	1·65	23 0
10	0 35 14·18	48·23	0·49	2 21 3·27	15·66	2·03	1 49 19·64	36·18	1·64	50
20	0 36 2·41	48·16	0·50	2 21 18·93	15·40	2·04	1 48 43·46	36·41	1·63	40
30	0 36 50·57	48·10	0·52	2 21 34·33	15·14	2·04	1 48 7·05	36·64	1·63	30
40	0 37 38·67	48·03	0·53	2 21 49·47	14·83	2·04	1 47 30·41	36·88	1·62	20
50	0 38 26·70	47·96	0·54	2 22 4·30	14·59	2·05	1 46 53·53	37·12	1·61	10
	Signs 0.— and VI.—			Signs I — and VII.—			Signs II.— and VIII.—			

TABLE continued.

Argument.	Signs 0.—— and VI.——			Signs I.—— and VII.——			Signs II.—— and VIII.——			Argument.
Long. Eclip.	Reduction.	Diff.	Var. Obl.	Reduction.	Diff.	Var. Obl.	Reduction.	Diff.	Var. Obl.	Long. Eclip.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
8 0	0 39 14·66	47·89	0·55	2 22 18·89	14·29	2·05	1 46 16·41	37·38	1·60	22 0
10	0 40 2·55	47·82	0·56	2 22 33·18	13·98	2·06	1 45 39·03	37·59	1·59	50
20	0 40 50·37	47·75	0·57	2 22 47·16	13·73	2·06	1 45 1·44	37·84	1·58	40
30	0 41 38·12	47·66	0·58	2 23 0·89	13·45	2·07	1 44 23·60	38·05	1·58	30
40	0 42 25·78	47·60	0·59	2 23 14·34	13·18	2·07	1 43 45·55	38·30	1·57	20
50	0 43 13·38	47·52	0·60	2 23 27·52	12·87	2·08	1 43 7·25	38·54	1·56	10
9 0	0 44 0·90	47·44	0·62	2 23 40·39	12·61	2·08	1 42 28·71	38·73	1·55	21 0
10	0 44 48·34	47·35	0·63	2 23 53·00	12·32	2·08	1 41 49·98	38·98	1·54	50
20	0 45 35·69	47·28	0·64	2 24 5·32	12·02	2·09	1 41 11·00	39·22	1·53	40
30	0 46 22·97	47·19	0·65	2 24 17·34	11·77	2·09	1 40 31·78	39·43	1·52	30
40	0 47 10·16	47·10	0·66	2 24 29·11	11·45	2·09	1 39 52·35	39·65	1·51	20
50	0 47 57·26	47·02	0·67	2 24 40·56	11·19	2·10	1 39 12·70	39·88	1·50	10
10 0	0 48 44·28	46·94	0·68	2 24 51·75	10·89	2·10	1 38 32·82	40·10	1·49	20 0
10	0 49 31·22	46·85	0·70	2 25 2·64	10·59	2·10	1 37 52·72	40·32	1·48	50
20	0 50 18·07	46·75	0·71	2 25 13·23	10·33	2·10	1 37 12·40	40·53	1·47	40
30	0 51 4·82	46·66	0·72	2 25 23·56	10·01	2·11	1 36 31·87	40·76	1·46	30
40	0 51 51·48	46·56	0·73	2 25 33·57	9·76	2·11	1 35 51·11	40·95	1·45	20
50	0 52 38·04	46·48	0·74	2 25 43·33	9·44	2·11	1 35 10·16	41·17	1·44	10
11 0	0 53 24·52	46·38	0·75	2 25 52·77	9·16	2·12	1 34 28·99	41·40	1·43	19 0
10	0 54 10·90	46·28	0·76	2 26 1·93	8·88	2·12	1 33 47·59	41·61	1·42	50
20	0 54 57·18	46·18	0·77	2 26 10·81	8·59	2·12	1 33 5·98	41·81	1·41	40
30	0 55 43·36	46·08	0·78	2 26 19·40	8·30	2·1[illegible]	1 32 24·17	42·02	1·40	30
40	0 56 29·44	45·98	0·79	2 26 27·70	7·97	2·13	1 31 42·15	42·23	1·39	20
50	0 57 15·42	45·88	0·80	2 26 35·67	7·72	2·13	1 30 59·92	42·42	1·38	10
12 0	0 58 1·30	45·75	0·82	2 26 43·39	7·42	2·13	1 30 17·50	42·63	1·37	18 0
10	0 58 47·05	45·67	0·83	2 26 50·81	7·12	2·13	1 29 34·87	42·84	1·36	50
20	0 59 32·72	45·57	0·84	2 26 57·93	6·81	2·14	1 28 52·03	43·04	1·35	40
30	1 0 18·29	45·44	0·85	2 27 4·74	6·55	2·14	1 28 8·99	43·25	1·33	30
40	1 1 3·73	45·34	0·86	2 27 11·29	6·24	2·14	1 27 25·74	43·43	1·32	20
50	1 1 49·07	45·22	0·87	2 27 17·53	5·93	2·14	1 26 42·31	43·63	1·31	10
13 0	1 2 34·29	45·11	0·88	2 27 23·46	5·66	2·15	1 25 58·68	43·81	1·30	17 0
10	1 3 19·40	45·01	0·89	2 27 29·12	5·37	2·15	1 25 14·87	44·01	1·29	50
20	1 4 4·41	44·88	0·90	2 27 34·49	5·04	2·15	1 24 30·86	44·23	1·28	40
30	1 4 49·29	44·76	0·91	2 27 39·53	4·77	2·15	1 23 46·63	44·38	1·26	30
40	1 5 34·05	44·65	0·92	2 27 44·30	4·49	2·15	1 23 2·25	44·59	1·25	20
50	1 6 18·70	44·51	0·93	2 27 48·79	4·11	2·15	1 22 17·66	44·77	1·24	10
14 0	1 7 3·21	44·41	0·94	2 27 52·83	3·98	2·15	1 21 32·89	44·96	1·23	16 0
10	1 7 47·62	44·29	0·95	2 27 56·81	3·57	2·16	1 20 47·93	45·13	1·22	50
20	1 8 31·91	44·15	0·96	2 28 0·38	3·21	2·16	1 20 2·80	45·31	1·21	40
30	1 9 16·06	44·04	0·97	2 28 3·62	3·02	2·16	1 19 17·49	45·50	1·20	30
40	1 10 0·10	43·91	0·98	2 28 6·64	2·66	2·16	1 18 31·99	45·68	1·19	20
50	1 10 44·01	43·77	0·99	2 28 9·30	2·40	2·16	1 17 46·31	45·84	1·18	10
15 0	1 11 27·78	43·65	1·00	2 28 11·70	2·07	2·17	1 17 0·47	46·05	1·17	15 0
10	1 12 11·43	43·51	1·01	2 28 13·77	1·76	2·17	1 16 14·42	46·20	1·16	50
20	1 12 54·94	43·39	1·02	2 28 15·53	1·47	2·17	1 15 28·22	46·36	1·15	40
30	1 13 38·33	43·26	1·03	2 28 17·00	1·21	2·17	1 14 41·86	46·55	1·13	30
40	1 14 21·59	43·11	1·04	2 28 18·21	0·86	2·17	1 13 55·31	46·71	1·12	20
50	1 15 4·70	42·99	1·05	2 28 19·07	0·60	2·17	1 13 8·60	46·88	1·11	10
	Signs 0.— and VI.—			Signs I.— and VII.—			Signs II. and VIII.			

METEOROLOGICAL TABLE,
BY MR. CARY, OF THE STRAND,
For June 1821.

Days of Month. 1821.	Thermometer. 8 o'Clock Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Weather.
May 27	47	54	44	29·97	Small showers
28	44	55	44	30·02	Small showers
29	47	57	45	·18	Fair
30	50	59	44	·32	Fair
31	47	59	50	·20	Fair
June 1	55	70	56	·13	Fair
2	55	70	57	·07	Fair
3	56	69	54	29·96	Fair
4	51	65	59	·71	Rain in the morng.
5	57	70	60	·82	Fair [cloudy.
6	60	69	57	30·00	Fair
7	57	65	54	29·75	Showery
8	50	54	46	·80	Showery
9	50	58	44	·85	Showery
10	45	50	42	·84	Showery
11	47	55	47	·98	Fair
12	50	53	47	30·33	Cloudy
13	55	56	48	·33	Cloudy
14	50	59	50	·40	Fair
15	52	65	51	·39	Fair
16	50	61	50	·31	Cloudy
17	50	58	51	·36	Cloudy
18	51	63	52	·41	Fair
19	52	65	49	·30	Fair
20	52	60	48	·16	Fair
21	50	59	49	·17	Fair
22	51	57	51	·26	Cloudy
23	52	55	48	·25	Cloudy
24	50	56	52	·17	Cloudy
25	52	62	51	·21	Cloudy
26	55	66	51	·17	Fair

N.B. The Barometer's height is taken at one o'clock.

Observations for Correspondent who observed the

10th June 8 o'Clock	M.	Barom. 29·80	Ther. attached 49°	Detached 45
— — 9	— — —	29·81	— — — 49	— — 49
— — 1	— N. —	29·84	— — — 50	— — 50

TABLE continued.

Argument.	Signs 0—— and VI.——			Signs I.—— and VII.——			Signs II.—— and VIII.——			Argument.
Long. Eclip.	Reduction.	Diff.	Var. Obl.	Reduction.	Diff.	Var. Obl.	Reduction.	Diff.	Var. Obl.	Long. Eclip.
° ′	° ′ ″	″	″	° ′ ″	″	″	° ′ ″	″	″	° ′
24 0	1 47 4·35	31·68	1·52	2 22 51·12	14·26	2·12	0 32 11·27	52·79	0·49	6 0
10	1 47 39·03	34·51	1·53	2 22 36·86	14·56	2·12	0 31 18·48	52·87	0·47	50
20	1 48 13·54	34·27	1·54	2 22 22·30	14·88	2·12	0 30 25·61	52·93	0·46	40
30	1 48 47·81	34·12	1·54	2 22 7·42	15·16	2·11	0 29 32·68	53·00	0·45	30
40	1 49 21·93	33·87	1·55	2 21 52·26	15·46	2·11	0 28 39·68	53·06	0·43	20
50	1 49 55·80		1·56	2 21 36·80		2·11	0 27 46·62		0·42	10
		33·70			15·75			53·13		
25 0	1 50 29·50	33·47	1·57	2 21 21·05	16·07	2·10	0 26 53·49	53·19	0·41	5 0
10	1 51 2·97	33·27	1·58	2 21 4·98	16·37	2·10	0 26 0·30	53·25	0·39	50
20	1 51 36·24	33·06	1·59	2 20 48·61	16·66	2·09	0 25 7·05	53·31	0·38	40
30	1 52 9·30	32·86	1·60	2 20 31·95	16·93	2·09	0 24 13·74	53·37	0·36	30
40	1 52 42·16	32·63	1·60	2 20 15·02	17·27	2·08	0 23 20·37	53·41	0·35	20
50	1 53 14·79		1·61	2 19 57·75		2·08	0 22 26·96		0·34	10
		32·43			17·54			53·48		
26 0	1 53 47·22	32·21	1·62	2 19 40·21	17·83	2·07	0 21 33·48	53·51	0·33	4 0
10	1 54 19·43	32·02	1·63	2 19 22·38	18·15	2·07	0 20 39·97	53·57	0·31	50
20	1 54 51·45	31·77	1·64	2 19 4·23	18·43	2·07	0 19 46·40	53·62	0·30	40
30	1 55 23·22	31·58	1·65	2 18 45·80	18·73	2·06	0 18 52·78	53·65	0·28	30
40	1 55 54·80	31·35	1·65	2 18 27·67	19·05	2·06	0 17 59·13	53·70	0·27	20
50	1 56 26·15		1·66	2 18 8·02		2·05	0 17 5·43		0·26	10
		31·14			19·31			53·74		
27 0	1 56 57·29	30·91	1·67	2 17 48·71	19·62	2·05	0 16 11·69	53·78	0·25	3 0
10	1 57 28·20	30·71	1·68	2 17 29·09	19·89	2·04	0 15 17·91	53·81	0·23	50
20	1 57 58·91	30·46	1·68	2 17 9·20	20·22	2·04	0 14 24·10	53·84	0·22	40
30	1 58 29·37	30·26	1·69	2 16 48·98	20·49	2·03	0 13 30·26	53·88	0·21	30
40	1 58 59·63	30·02	1·70	2 16 28·49	20·78	2·03	0 12 36·38	53·91	0·19	20
50	1 59 29·65		1·70	2 16 7·71		2·02	0 11 42·47		0·18	10
		29·80			21·09			53·93		
28 0	1 59 59·45	29·59	1·71	2 15 46·62	21·35	2·02	0 10 48·54	53·95	0·17	2 0
10	2 0 29·04	29·36	1·72	2 15 25·27	21·67	2·91	0 9 54·59	53·99	0·15	50
20	2 0 58·40	29·11	1·72	2 15 3·60	21·93	2·01	0 9 0·60	54·00	0·14	40
30	2 1 27·51	28·89	1·73	2 14 41·67	22·24	2·00	0 8 6·60	54·02	0·13	30
40	2 1 56·40	28·66	1·74	2 14 19·43	22·52	2·00	0 7 12·58	54·04	0·11	20
50	2 2 25·06		1·74	2 13 56·91		1·99	0 6 18·54		0·10	10
		28·42			22·83			54·04		
29 0	2 2 53·48	28·21	1·75	2 13 34·08	23·08	1·98	0 5 24·50	54·07	0·08	1 0
10	2 3 21·69	27·98	1·76	2 13 11·00	23·39	1·98	0 4 30·43	54·07	0·07	50
20	2 3 49·67	27·72	1·77	2 12 47·61	23·67	1·97	0 3 36·36	54·08	0·05	40
30	2 4 17·39	27·49	1·77	2 12 23·91	23·94	1·97	0 2 42·28	54·09	0·04	30
40	2 4 44·88	27·26	1·78	2 12 0·00	24·26	1·96	0 1 48·19	54·10	0·02	20
50	2 5 12·14	27·02	1·79	2 11 35·71	24·50	1·96	0 0 54·09	54·09	0·01	10
30 0	2 5 39·16		1·80	2 11 11·24		1·95	0 0 0·0		0·00	0 0
Long. of the Eclip.	Signs V.+ and XI.+			Signs IV.+ and X.+			Signs III.+ and IX.+			Long. of the Eclip.

Enter the Table with the Longitude of the Ecliptic as the Argument, and take out the corresponding reduction, applying the correction for the variation of the Obliquity according as it is more or less than 23° 27′ 57″. This Equation applied according to its sign to the Longitude of the point of the Ecliptic gives the Sun's Right Ascension as required.

LXXXII. *Vari-*

LXXXII. *Variation of the ⊙'s R. A. and Declin. for* 100″ *Diminution of the Obliquity of the Ecliptic.*

Argu. ⊙'s Long.	Signs 0 and VI. Var. R. A	Var. Dec.	Signs I. and VII. Var. R. A.	Var. Dec	Signs II and VIII. Var. R. A.	Var. Dec.	Argu. ⊙'s Long.
° ′	″	″	″	″	″	″	° ′
0 0	0·00	0·00	17·92	46·81	19·56	84·64	30 0
10	0·12	0·27	17·99	47·06	19·50	84·80	50
20	0·23	0·53	18·06	47·30	19·44	84·96	40
30	0·35	0·80	18·13	47·55	19·37	85·12	30
40	0·47	1·07	18·20	47·80	19·31	85·28	20
50	0·58	1·33	18·27	48·04	19·25	85·44	10
1 0	0·70	1·60	18·34	48·29	19·19	85·60	29 0
10	0·82	1·87	18·40	48·53	19·13	85·76	50
20	0·93	2·13	18·47	48·77	19·07	85·91	40
30	1·05	2·40	18·53	49·01	19·00	86·07	30
40	1·16	2·67	18·59	49·25	18·94	86·22	20
50	1·28	2·93	18·65	49·49	18·88	86·38	10
2 0	1·39	3·20	18·71	49·73	18·82	86·53	28 0
10	1·51	3·47	18·77	49·97	18·75	86·69	50
20	1·62	3·73	18·83	50·22	18·68	86·84	40
30	1·74	4·00	18·89	50·46	18·62	86·99	30
40	1·85	4·27	18·95	50·70	18·55	87·14	20
50	1·97	4·53	19·01	50·94	18·48	87·29	10
3 0	2·08	4·80	19·07	51·18	18·41	87·44	27 0
10	2·20	5·07	19·13	51·42	18·33	87·59	50
20	2·31	5·33	19·18	51·66	18·26	87·73	40
30	2·43	5·60	19·24	51·90	18·18	87·88	30
40	2·54	5·87	19·30	52·14	18·11	88·02	20
50	2·66	6·13	19·35	52·38	18·03	88·17	10
4 0	2·77	6·40	19·41	52·62	17·96	88·31	26 0
10	2·89	6·67	19·46	52·86	17·88	88·45	50
20	3·00	6·93	19·52	53·10	17·81	88·59	40
30	3·12	7·20	19·57	53·34	17·74	88·73	30
40	3·23	7·47	19·62	53·58	17·66	88·87	20
50	3·35	7·73	19·67	53·82	17·59	89·01	10
5 0	3·46	8·00	19·72	54·06	17·52	89·15	25 0
10	3·58	8·27	19·77	54·30	17·44	89·29	50
20	3·69	8·53	19·82	54·53	17·36	89·43	40
30	3·81	8·80	19·87	54·77	17·27	89·57	30
40	3·92	9·07	19·92	55·00	17·19	89·70	20
50	4·04	9·33	19·97	55·24	17·11	89·84	10
6 0	4·15	9·60	20·02	55·47	17·03	89·98	24 0
10	4·26	9·87	20·06	55·70	16·95	90·11	50
20	4·38	10·13	20·11	55·94	16·86	90·24	40
30	4·49	10·40	20·15	56·17	16·78	90·37	30
40	4·60	10·66	20·19	56·40	16·70	90·50	20
50	4·72	10·92	20·23	56·64	16·61	90·63	10
7 0	4·83	11·19	20·27	56·87	16·53	90·76	23 0
10	4·95	11·46	20·31	57·10	16·45	90·89	50
	Signs V and XI.		Signs IV and X.		Signs III. and IX		

Argu. ⊙'s Long.	Signs 0 and VI.		Signs I. and VII.		Signs II. and VIII.		Argu. ⊙'s Long.
	Var. R. A.	Var. Dec.	Var. R. A.	Var. Dec.	Var. R. A.	Var. Dec.	
° ′	″	″	″	″	″	″	° ′
7 20	5·06	11·72	20·36	57·34	16·36	91·02	23 40
30	5·17	11·99	20·40	57·57	16·27	91·15	30
40	5·28	12·25	20·44	57·81	16·19	91·28	20
50	5·49	12·52	20·49	58·04	16·10	91·40	10
8 0	5·50	12·78	20·53	58·27	16·01	91·53	22 0
10	5·61	13·05	20·57	58·50	15·92	91·65	50
20	5·72	13·31	20·60	58·72	15·82	91·77	40
30	5·84	13·58	20·64	58·95	15·73	91·90	30
40	5·95	13·85	20·68	59·18	15·63	92·02	20
50	6·06	14·11	20·71	59·40	15·54	92·14	10
9 0	6·17	14·38	20·75	59·63	15·44	92·26	21 0
10	6·28	14·64	20·78	59·86	15·35	92·38	50
20	6·39	14·91	20·82	60·09	15·25	92·49	40
30	6·50	15·17	20·85	60·31	15·15	92·61	30
40	6·61	15·44	20·89	60·54	15·05	92·73	20
50	6·72	15·70	20·92	60·77	14·95	92·84	10
10 0	6·83	15·97	20·96	61·00	14·85	92·96	20 0
10	6·94	16·23	20·99	61·23	14·76	93·07	50
20	7·05	16·50	21·02	61·45	14·66	93·19	40
30	7·17	16·76	21·05	61·68	14·56	93·30	30
40	7·28	17·03	21·08	61·90	14·47	93·41	20
50	7·39	17·29	21·11	61·13	14·37	93·53	10
11 0	7·50	17·56	21·14	62·36	14·27	93·64	19 0
10	7·61	17·82	21·16	62·58	14·17	93·74	50
20	7·72	18·09	21·18	62·80	14·06	93·85	40
30	7·83	18·35	21·20	63·03	13·96	93·95	30
40	7·94	18·61	21·23	63·25	13·86	94·06	20
50	8·05	18·88	21·25	63·47	13·76	94·16	10
12 0	8·16	19·14	21·27	63·69	13·66	94·27	18 0
10	8·26	19·40	21·29	63·91	13·55	94·37	50
20	8·37	19·67	21·32	64·13	13·44	94·47	40
30	8·47	19·93	21·34	64·35	13·33	94·57	30
40	8·58	20·19	21·36	64·57	13·22	94·67	20
50	8·68	20·46	21·39	64·79	13·11	94·77	10
13 0	8·79	20·72	21·41	65·01	13·00	94·87	17 0
10	8·89	20·98	21·43	65·23	12·89	94·97	50
20	9·00	21·25	21·45	65·45	12·78	95·06	40
30	9·10	21·51	21·47	65·66	12·67	95·16	30
40	9·21	21·77	21·49	65·88	12·57	95·26	20
50	9·31	22·04	21·51	66·10	12·46	95·36	10
14 0	9·42	22·30	21·53	66·32	12·35	95·45	16 0
10	9·52	22·56	21·54	66·53	12·24	95·54	50
20	9·63	22·83	21·56	66·75	12·13	95·64	40
30	9·73	23·09	21·57	66·96	12·01	95·73	30
40	9·84	23·35	21·59	67·18	11·90	95·82	20
50	9·94	23·62	21·60	67·39	11·79	95·91	15 10
	Signs V. and XI.		Signs IV. and X.		Signs III. and IX.		

Argu-

Argu. ☉'s Long.	Signs 0 and VII.		Signs I. and VII.		Signs II. and VIII.		Argu. ☉'s Long.
	Var. R.A.	Var. Dec.	Var. R.A.	Var. Dec.	Var. R.A.	Var. Dec.	
° ′	″	″	″	″	″	″	° ′
15 0	10·05	23·88	21·61	67·61	11·68	96·00	15 0
10	10·15	24·14	21·62	67·82	11·56	96·08	50
20	10·26	24·40	21·63	68·03	11·44	96·17	40
30	10·36	24·67	21·64	68·25	11·33	96·25	30
40	10·47	24·93	21·65	68·46	11·21	96·33	20
50	10·57	25·19	21·66	68·67	11·09	96·42	10
16 0	10·68	25·45	21·67	68·88	10·97	96·50	14 0
10	10·78	25·71	21·67	69·09	10·85	96·58	50
20	10·88	25·97	21·67	69·30	10·73	96·67	40
30	10·98	26·23	21·67	69·51	10·62	96·75	30
40	11·08	26·49	21·67	69·72	10·50	96·83	20
50	11·18	26·75	21·67	69·93	10·38	96·92	10
17 0	11·28	27·01	21·67	70·14	10·26	97·00	13 0
10	11·38	27·27	21·67	70·35	10·14	97·07	50
20	11·47	27·53	21·67	70·56	10·02	97·15	40
30	11·57	27·79	21·67	70·76	9·89	97·22	30
40	11·67	28·05	21·67	70·97	9·77	97·30	20
50	11·76	28·31	21·67	71·18	9·65	97·37	10
18 0	11·86	28·57	21·67	71·39	9·53	97·44	12 0
10	11·96	28·83	21·66	71·59	9·41	97·50	50
20	12·06	29·09	21·66	71·80	9·29	97·57	40
30	12·15	29·35	21·65	72·00	9·16	97·64	30
40	12·25	29·60	21·65	72·20	9·04	97·70	20
50	12·35	29·86	21·64	72·40	8·91	97·77	10
19 0	12·45	30·12	21·64	72·60	8·79	97·84	11 0
10	12·54	30·38	21·63	72·80	8·67	97·90	50
20	12·64	30·64	21·62	73·00	8·54	97·97	40
30	12·74	30·90	21·62	73·20	8·42	98·03	30
40	12·83	31·15	21·61	73·40	8·29	98·09	20
50	12·93	31·41	21·60	73·60	8·17	98·15	10
20 0	13·03	31·67	21·59	73·80	8·04	98·21	10 0
10	13·12	31·93	21·58	73·99	7·91	98·27	50
20	13·22	32·19	21·57	74·19	7·79	98·32	40
30	13·31	32·45	21·56	74·38	7·66	98·38	30
40	13·40	32·71	21·55	74·58	7·53	98·44	20
50	13·49	32·96	21·53	74·77	7·41	98·49	10
21 0	13·58	33·22	21·52	74·97	7·28	98·55	9 0
10	13·67	33·48	21·50	75·16	7·15	98·60	50
20	13·77	33·73	21·48	75·36	7·02	98·66	40
30	13·86	33·99	21·46	75·55	6·88	98·71	30
40	13·95	34·25	21·44	75·75	6·75	98·76	20
50	14·04	34·50	21·41	75·94	6·62	98·81	10
22 0	14·13	34·76	21·39	76·14	6·49	98·86	8 0
10	14·22	35·01	21·37	76·33	6·36	98·90	50
20	14·31	35·27	21·35	76·52	6·23	98·95	40
30	14·40	35·52	21·34	76·71	6·10	98·99	30
	Signs V. and XI.		Signs IV. and X.		Signs III. and IX.		

Argu-

Arg. ⊙'s Long.	Signs 0. and VI. Var. R. A.	Signs 0. and VI. Var. Dec.	Signs I. and VII. Var. R. A.	Signs I. and VII. Var. Dec.	Signs II. and VIII. Var. R. A.	Signs II. and VIII. Var. Dec.	Arg. ⊙'s Long.
	″	″	″	″	″	″	° ′
	14·49	35·78	21·32	76·90	5·96	99·03	20
	14·58	36·03	21·30	77·09	5·83	99·08	10
23	14·67	36·29	21·28	77·28	5·70	99·12	0
10	14·75	36·54	21·25	77·47	5·57	99·16	50
20	14·84	36·80	21·23	77·65	5·44	99·20	40
30	14·92	37·05	21·20	77·84	5·31	99·24	30
40	15·01	37·31	21·18	78·03	5·18	99·28	20
50	15·09	37·56	21·15	78·21	5·05	99·32	10
4 0	15·18	37·82	21·12	78·40	4·92	99·36	6 0
10	15·26	38·07	21·08	78·58	4·78	99·40	50
20	15·35	38·33	21·05	78·77	4·65	99·43	40
30	15·43	38·58	21·01	78·95	4·51	99·46	30
40	15·52	38·83	20·98	79·14	4·37	99·49	20
50	15·60	39·09	20·94	79·32	4·24	99·52	10
5 0	15·69	39·34	20·91	79·51	4·10	99·55	0
10	15·77	39·60	20·87	79·69	3·96	99·58	50
20	15·85	39·85	20·83	79·86	3·83	99·60	40
30	15·93	40·10	20·78	80·04	3·69	99·63	30
40	16·01	40·35	20·74	80·22	3·55	99·66	20
50	16·09	40·60	20·70	80·39	3·42	99·69	10
6 0	16·17	40·85	20·66	80·57	3·28	99·72	4 0
10	16·25	41·10	20·62	80·75	3·15	99·74	50
20	16·33	41·35	20·58	80·93	3·01	99·76	40
30	16·40	41·60	20·54	81·10	2·88	99·79	30
40	16·48	41·85	20·50	81·28	2·74	99·81	20
50	16·56	42·10	20·46	81·46	2·61	99·83	10
7 0	16·64	42·35	20·42	81·64	2·47	99·85	3 0
10	16·71	42·60	20·38	81·81	2·33	99·86	50
20	16·79	42·85	20.33	81·98	2·20	99·88	40
30	16·86	43·10	20·29	82·15	2·06	99·89	30
40	16·94	43·35	20·25	82·32	1·92	99·91	20
50	17·01	43·60	20·20	82·49	1·79	99·92	10
0	17·08	43·85	20·16	82·66	1·65	99·94	2 0
10	17·15	44·10	20·11	82·83	1·51	99·95	50
20	17·22	44·35	20·06	82·99	1·38	99·96	40
30	17·29	44·60	20·01	83·16	1·24	99·96	30
40	17·36	44·85	19·96	83·33	1·11	99·97	20
50	17·43	45·10	19·91	83·49	0·97	99·97	10
0	17·52	45·34	19·86	83·66	0·83	99·98	1 0
10	17·59	45·59	19·81	83·82	0·70	99·98	50
20	17·65	45·83	19·76	83·99	0·56	99·99	40
30	17·72		19·71	84·15	0·42	99·99	30
40	17·79		19·66	84·31	0·28	99·99	20
50	17·85	46·57	19·61	84·48	0·14	100·00	10
0	17·92	46·81	19·56	84·64	0·00	100·00	0 0
	igns V.		IV. and X.		gns III. and IX.		

Note.

Note.—In whatever manner M. Burkhardt combined the observations of Lacaille, Mayer, Bradley, Le-Gentil, Maskelyne, Piazzi and his own, he could never obtain more than 48″, for the Secular Variation of the Obliquity, he has concluded the Secular Diminution cannot be greater than 50″, he has never believed that it amounted to 52″;—Lagrange states the Diminution at 56″ per century; Laplace, and Delambre in his Solar Tables, 52″·1. In the 2d edition of Vince's Astronomy, from a mean of the observations of Dr. Maskelyne and several other astronomers, it is stated at 50″; whereas Dr. Brinkley makes it 43″ only. *Vide* Phil. Mag. vol. 56, p. 213.

Norfolk-street, Lynn Regis, June 4, 1821. J. Utting.

To the Editor of the Philosophical Magazine.

Lynn, June 5, 1821.

Dear Sir,—I have sent you a supplementary Table*, containing the Variation of the Sun's Right Ascension and Declination for 100 seconds Diminution of the Obliquity of the Ecliptic, in preference "to a Diminution of 60″, after the example of Mayer," as suggested by your Correspondent in the Philosophical Magazine for last month (p. 395 of this volume): for, in finding the proportional parts of obliquities differing from that assumed in the construction of the Tables, the operation of dividing by the first term is unnecessary, and is in my Tables dispensed with: attention only is required in pointing off the decimal places. The error in the Table of the Variation of the Sun's Right Ascension in time cannot, at its maximum, exceed about one-tenth of a second in a century: any further correction is therefore unnecessary.

Such persons as choose to have the Tables copied for their use can easily substitute the latter variations as the Diminution of the Obliquity, for the Secular Variation originally inserted in the Tables.

I remain, yours truly,

J. Utting.

* The Table alluded to, is that occupying the four pages immediately preceding this letter.—Edit.

 LXXXIII. *On*

LXXXIII. *On the Passage of the Comet of* 1819 *across the Disc of the Sun**. *By M.* Olbers.

All the elements indicated this passage. According to those of Dirksen, the immersion was to take place at 17h 30′ 34″, and the emersion at 21h 5′ 37″ on the 25th June 1819; the hours were reckoned according to true time, Milan meridian. At 19h 18′ 6″ the centre of the comet was only removed from that of the sun 2′ 8″. The aberration retards these phases 5′ 31″ and the parallax half a minute for the observatories of Germany.

In the Ephemeris of Berlin for 1822, I concluded (says M. Olbers) from the observation of M. de Lindener, confirmed by an observation made in Austria, that the comet had been invisible on the disc of the sun; but it is now proved that the sun was not without spots at that epoch; and if these spots were not remarked by the two observers to whom I have alluded, the comet, much more difficult to be seen, may also have escaped their notice, although a more attentive or more piercing eye might have been able to discern it. The following are incontestable proofs of the existence of the spots.

(1). M. Schumaker, then at Altona, had determined the collimation of his Troughton's sextant several times in the course of the month of June; and among others, on the 25th at 20h. He recollects most positively of never having seen the sun without spots. The glass of the sextant multiplied nearly ten times; it is therefore not probable that one of these spots could be the comet.

(2.) Professor Brandes at Breslau viewed the sun on the 26th of June, a little before midday, with a glass of a thirty-four times multiplying power, and he perceived a spot distinctly visible, almost passing behind the disc, and precisely at the place where it ought to have been according to the preceding observations. This observation becomes important when compared with that of Dr. Gruithuisen inserted in the *Gazette Politique* of Munich, of the 12th of August 1819. According to the Meteorological Journal of Dr. Gruithuisen, there were on the 26th of June, at eight in the morning, two small spots without nebulosity near the west limb of the sun. One also was seen in the midst of the apparent disc. As well as he can recollect, the spot in the middle was very small and undefined; it is possible, therefore, that this philosopher might have seen the comet on the disc of the sun. Nevertheless doubts must still remain, until we learn that some other observer has seen this black point, either forward

* Extracted from a Memoir by M. Olbers in the Ephemeris of Berlin for 1823.

near

near the south limb, or behind near the north limb, considering that spots have never been seen in the neighbourhood of the poles of the sun. This spot in the middle appeared somewhat larger than double the size of the fourth satellite of Jupiter; it was not an old spot; indeed, four days before M. Gruithuisen had observed the following spots:

"Near the west limb a large spot with nebulosity.

"Towards the middle, but still a little to the west, three new spots rather large, and some smaller ones.

"And very near the east limb a small spot.

"The large spots in the middle were on the 26th of June near the west limb, and had greatly diminished; that on the east limb had disappeared; at least it is certain that it had not arrived at the middle of the sun in four days. It follows, that the small black spot in the middle of the disc on the 26th of June was a new spot, or the nucleus of the comet."

The observations of MM. Brandes and Gruithuisen appear to indicate that between the 23d and 28th of June none of the ordinary spots seen before or after that period had reached the middle of the solar disc; and the observation made at Hanover by Professor Wildt, appears to confirm that made on the 26th of June by M. Gruithuisen.

"I observed the sun," says M. Wildt, "about the 26th of June, one or two days sooner or later, and I saw an undefined spot, of which I still recollect the size and the situation. I believe it was the comet I saw upon the sun; my observation was made towards seven in the morning, and might have indicated the situation and the size of the comet. It is perhaps the only time that I did not commit my observation to writing; but the spot was so faint, and so indeterminate, that it appeared to me of little interest. It is truly to be regretted that the idea of a comet had not occurred to me. The fact of not having written down my observation leads me to think that it must have been on the 26th of June; for I am aware of circumstances which might have deprived me of the opportunity on that day. If then the calculations of M. Olbers were confirmed by my observations, I should be inclined to believe that it was the comet which I saw on the sun's disc."

"I will not decide," adds M. Olbers, "whether what MM. Gruithuisen and Wildt saw upon the sun (at the period of the passage) was really the comet, or an ordinary spot. However, none of the spots seen before the 26th of June could be on that day in the middle of the disc; and M. Brandes could not perceive at twelve o'clock the spot observed at eight by M. Gruithuisen: these two facts appear very remarkable. It is to be regretted, that there has not been any decisive observation of a phænomenon so interesting

interesting and so rare; but it has appeared to me proper, nevertheless, to collect here all the observations on the solar disc made on the 26th of June 1819, of which I have been able to receive an account."

LXXXIV. *Electro-magnetic Experiments.* By Mr. J. Tatum.

Dorset-street, May 17, 1821.

Sir, — If you are of opinion that the following arrangement of Electro-magnetic Apparatus, and experiments performed with it, are worthy of a place in your publication, they are at your service. I am, sir, yours, &c.

J. Tatum.

To the Editor of the Phil. Mag.

Let A, B, C and D (fig. A, Plate IV) represent a copper vessel about four inches by three, and about half an inch wide, in which is inserted a plate of zinc *z z*, similar to that of Professor Œrsted, as described in your Magazine for January, page 46, excepting only that that was suspended by means of a loop of thread, and this is supported by a fine point, at the bottom, and one at the top of the copper vessel, so as to allow it to rotate freely when put in action by a very small power. Let E, F, G, H and I represent a copper wire soldered to the plate of zinc at E, and continued to F, where it is bent at a right angle, and continued to G, H, and I, where it is soldered to one side of the copper vessel.

Let the copper vessel be filled with diluted nitro-sulphuric acid, and the following phænomena will be observed.

1. On bringing the north pole of a strong magnetic bar under the copper wire near G, the apparatus will be strongly repelled, and rotate quickly on its axis.

2. If the south pole of the same bar be presented, the apparatus will as quickly rotate by attraction.

3. On bringing the north pole above the wire near F, the apparatus will rotate by attraction.

4. But on approaching the same part of the wire, with the south pole of the magnet, the apparatus will rotate by repulsion.

5. If some fine iron filings be sprinkled or dropt on the wire, or if a piece of paper containing such be brought under the same, so as it may touch them, they will adhere to it in little clots. This experiment succeeds if the wire be brass or platina.

6. If the north pole of a magnetic needle be brought under the wire, it is violently repelled.

7. If the south pole be presented to the same part, it will be attracted.

8. On

8. On bringing the north pole above the wire, it is attracted.
9. But the south pole will be repelled.

A friend who called on me, to see the action of the apparatus, observed, that if we should alter the position of the copper wire, so as to enable us to bring a small needle between it and the copper vessel, we should find that the under surface would either attract or repel the needle, according to the position in which it was held. Accordingly, on altering the apparatus, it was found to answer what he anticipated.

The above apparatus is well calculated to show the rotatory motion, as there is no twisting or untwisting of a thread, which will alter by the addition of weight or moisture.

LXXXV. *Notices respecting New Books.*

A Treatise on Scrofula (to which the Jacksonian Prize for the year 1818 was adjudged by the Court of Examiners of the Royal College of Surgeons); describing the Morbid Alteration it produces in the Structure of all the different Parts of the Body, and the best mode of treating it, particularly in Children; also its Connection with Diseases of the Spine, Joints, Eyes, and Glands, more especially of the Female Breasts, Testes, and Prostate Glands; with particular reference also to the most improved Plan of treating Spinal Curvatures. To which is added, an Account of the Ophthalmia, so long prevalent in Christ's Hospital. By Eusebius Arthur Lloyd, Member of the Royal College of Surgeons in London, &c. One volume 8vo.

A Dissertation on Lime, and its Use and Abuse in Agriculture. By Thomas Hornby. 8vo. 2*s*.

The British Botanist; or, a familiar Introduction to the Science of Botany. Fifteen Plates. 12mo. 7*s*. 6*d*.; coloured 10*s*. 6*d*.

The Botanical Cultivator; or, Instructions for the Management of Plants cultivated in the Hot-houses of Great Britain. By Robert Sweet, F.L.S. 8vo. 10*s*. 6*d*.

A Manual of the Diseases of the Human Eye, intended for Surgeons commencing Practice; translated from the German of Dr. Charles Hen. Weller, of Berlin. By G. C. Monteath, M.D. 2 vols. 8vo.; with four coloured Plates representing 37 diseased Eyes. 1*l*. 10*s*.

Illustrations of the Great Operations of Surgery, Trepan, Hernia, Amputation, Aneurism, and Lithotomy. By Charles Bell, F.R.S. E., containing 21 Plates. Large 4to, 3*l*. 15*s*.; coloured 5*l*. 5*s*.

A Treatise on the Medical Powers of the Nitro-muriatic Acid Bath, in various Diseases. By Walter Dunlop, Surgeon. 8vo. 2*s*.

A View

A View of the Structure, Functions and Disorders of the Stomach and Alimentary Organs of the Human Body; with physiological Observations and Remarks upon the Qualities and Effects of Food and fermented Liquors. By T. Hare. 8vo. 12*s.*

A Treatise on the Epidemic Cholera of India. By James Boyle. 8vo. 5*s.*

Practical Observations on those Disorders of the Liver, and other Organs of Digestion, which produce the several Forms and Varieties of the bilious Complaint. By Joseph Ayre, M.D. 8*s.* 6*d.*

A Description of Surgical Operations originally peculiar to the Japanese and Chinese, and by them denominated Zin King; now introduced into European Practice, with Directions, &c. By J. M. Churchill, Surgeon. 4*s.*

Familiar Lessons on Mineralogy and Geology. By J. Mawe. 12mo. 5*s.*

Preparing for Publication.

The First Volume (dedicated by Permission to His Majesty) of A General History of Birds, by John Latham, M.D. F.R.S. to be completed in Ten Volumes, demy 4to, with at least 180 coloured Plates, is intended to appear in a few days, and the succeeding volumes at intervals of about three months.

The Principles and Doctrines of Assurances, Annuities on Lives, and of Contingent Reversions, stated and explained. By W. Morgan, Esq. F.R.S. Actuary of the Equitable Life Insurance Office.

An Account of the Fossils of the South Downs; or Outlines of the Geology of the South-eastern Division of Sussex; in Royal 4to; illustrated by numerous Engravings. By Mr. Mantell, of Lewes.

A Journal of a Residence in the Burhmon Empire, and particularly at the Court of Amarapoora. By Capt. Cox.

The Conchology of the British Islands; a splendid work in 4to. By Dr. Turton. With 19 Plates.

The Parent's Medical and Surgical Assistant; intended for the use of the Heads of Families, parochial Clergymen, and others; affording familiar and popular Directions for the Management of the sudden Illness and various Accidents that require a prompt and judicious Treatment, and will not admit of the delay necessary for procuring advice. By Thomas Ayre Bromhead, M.B. Christ's College, Cambridge. In a small volume.

John Ayrton Paris, M.D. Fellow of the Royal College of Physicians, and John S. M. Fonblanque, Esq. Barrister at Law, have in considerable forwardness a work to be comprised in one volume in 8vo, and entitled "Medical Jurisprudence." It will comprehend Medical, Chemical, Anatomical and Surgical Investigations, applicable to Forensic Practice, for the Instruction and Guidance

of

of Coroners, Magistrates, Counsel, and Medical Witnesses: with a copious Appendix of Statutes, Cases and Decisions.

A Treatise of the Principles of Bridges by Suspension, with Reference to the Catenary, and exemplified by the Cable Bridge now in progress over the Strait of Menai. In it the Properties of the Catenary will be fully investigated, and those of Arches and Piers will be derived from the Motion of a Projectile. It will contain practical Tables, a Table of the Dimensions of a Catenary, and Tables of the principal Chain, Rope, Stone, Wood and Iron Bridges, with the Dimensions of them, erected in different Countries.

LXXXVI. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

June 8. THIS Society met for the last time this sessions, when several letters were read from foreign astronomers detailing some interesting particulars in the science. A communication was also made from the venerable president (Sir Wm. Herschel) of a new list of double stars. A paper was then read, from the Rev. Dr. Pearson, relative to his prismatic eye-piece; being, in some measure, a continuation of his former papers on this subject. —As this Society are now printing their Transactions, we shall abstain from noticing any further particulars: since the papers themselves will, in a short time, be before the public.

The Society has adjourned to Friday, November 9th.

PHRENOLOGICAL SOCIETY OF EDINBURGH.

This Society has just published a Report of its proceedings since its establishment on the 22d of Feb. 1820, to the close of the second Session on the 23d of April 1821, of which the following is a pretty copious abstract:

"The existence of this Society implies a belief in the members, that the brain is the organ of the mind, and that particular parts of it are the organs of particular mental faculties; and that these facts afford a key to the true philosophy of man. The Society is aware of the opposition which the doctrines have met with, and of the ridicule which has been cast upon them; but they know also, that in all ages a similar reception has been given to the most important discoveries; which, nevertheless, have in time prevailed.

"The propositions that Consciousness reveals nothing in regard to the seat or distribution of the organs of the mind, and Dissection nothing in regard to the functions of the brain, are so ob-

viously true, that they admit of no dispute. Nevertheless, philosophers on the mind, in conducting their inquiries, have relied too much on mere mental reflection; while physiologists, in seeking to discover the functions of the brain, have resorted too exclusively to dissection. These facts explain the past and present ignorance of mankind in general, on these interesting points in the philosophy of man; and point out, in the clearest manner, the necessity of resorting to a new method of inquiry, that more perfect information may be obtained. Dr. Gall was the first to introduce the mode of comparing mental manifestations with cerebral development; and this method has led to discoveries which could never have been attained by the means previously employed. By this mode of philosophising, the Phrenologist has attained knowledge in place of ignorance, and system in place of hypothesis, in many points highly important in the philosophy of the mind, and the physiology of the brain; and while he perceives, with a mixture of pity and regret, the determination with which many sensible men adhere to the former defective systems, he feels a perfect conviction that the new method requires only to be studied to be highly appreciated; and that as it becomes known the present opposition must disappear. The ridicule with which the new doctrines are pursued gives the Society no uneasiness. They have experience for their warrant in saying, that it is uniformly in the ratio of the ignorance of him from whom it proceeds; and they know that no enlightened individual ever compared mental manifestations and development of brain, with a serious desire to discover the truth, who found it possible to continue to scoff. Those who made the discoveries of Galileo and Newton objects of their wit, appear now to have been shallow-minded indeed; and those who have found subjects of ridicule in the constitution of their own nature, will probably be judged by posterity to have been not more profound. The Phrenologist knows that the organization and functions of the brain are as independent of human belief as the motions of the globe. Individuals, therefore, may scoff at the doctrines of phrenology, and think them too ridiculous to merit an inquiry: but the Society are convinced, that nevertheless every human being will continue, although unconsciously to himself, to manifest his faculties, by means of different parts of the brain, and with a power corresponding to their size and activity; and that mankind at large will believe the fact, as they did the revolution of the globe, whenever they turn their attention to the evidence. The Society, therefore, hold the principles of Phrenology as no longer subject to doubt; and while they recommend an attention to the subject to every reflecting and virtuous individual, who considers a knowledge of himself and of human nature as an object of importance,

they conceive themselves called upon to direct their own efforts rather to enlarge and apply the truths which Phrenology reveals, than to strengthen the evidence on which the general principles rest."

During the first season the following Essays were read:

"An Essay accounting for the Rise and Progress of Society, on Phrenological Principles, illustrated by Casts of the Skulls of Individuals of a Variety of Nations, in different Stages of Civilization; by Mr. G. Combe.

"On the different Species of Philosophy, and on the Facilities afforded by Phrenology for the Study of Man; by the Rev. David Welsh.

"On the Connection betwixt the Mind and the Brain; by Mr. James Brownlee.

"On Insanity, as illustrated by Phrenology. By Mr. Andrew Combe.

"On the Relation betwixt Metaphysics and Phrenology; by Mr. William Ritchie.

"On the Principles of Dramatic Compositions, as illustrated by the Views of the human Mind afforded by Phrenology; by Mr. G. Combe.

"Miss Clara Fisher, a child of nine years of age, having appeared on the Edinburgh stage, and exhibited great and precocious talent, the Society, through the kind permission of her father, procured a cast and a drawing of her head; and on 30th June, these, along with an analytical account of her mental manifestations, were presented to the Society by a Committee appointed for the purpose.

"On the Advantages to be derived from studying Man on Phrenological Principles; by the Rev. A. Stewart.

"August 1st, A Collection of Skulls of the lower Animals was presented and examined, and the Correspondence betwixt their Instincts, and the Development of their Heads, was pointed out, so far as known; by Dr. Robert Willis.

"This terminated the first session of the Society, which during this period consisted of ten members: the meetings were adjourned till November in the same year."

"During the second session, the following Essays were read:

"1. An Explanation of some Differences in Taste, on Phrenological Principles; by Sir G. S. Mackenzie.

"2. On the Causes of the Imperfection of Metaphysical Science, and on the Means of removing them; by Mr. G. Combe.

"3. Phrenological Observations on Haydon's Picture of Christ's Entry into Jerusalem; by Sir G. Mackenzie and Mr. A. Buchanan.

"4. On the Talents of eminent Men, as illustrated by their

cerebral Development; and, in particular, on the Genius of certain living Characters whose Development is known; by Mr. A. Buchanan.

"5. Further Observations on the Progress of Civilization in different Nations, as illustrated by Casts of their Skulls; by Mr. G. Combe.

"6. On the Tendency which Phrenology is supposed to have towards Materialism, and on the Means which it affords of accounting for the Diversity of Characters and intellectual Endowments observable among Mankind; by Mr. Brownlee.

"7. On Dr. Thomas Brown's System of Metaphysics and Morals, as connected with Phrenology; by Mr. Ritchie.

"8. Observations on Mr. Owen's Plan for banishing Vice and Misery from Society, as affected by the Doctrine of innate Dispositions, and on the other Means which remain of improving the Condition of the human Race; by Mr. G. Combe.

"One of the most obvious truths in the philosophy of man is, that the character and conduct of individuals are the results of their innate dispositions and talents exercised by themselves, and modified by the circumstances in which they are placed. It follows from this principle, that crimes arise from unfortunate natural dispositions; from neglected education; from the influence of unfavourable circumstances; or from the joint action of all these causes. The causes of crimes must be known before effectual measures can be adopted for their prevention; and hence it becomes an important object to discover, in what respect, or to what extent, the actions of criminals arise from natural tendencies, and to what extent from excitement produced by the circumstances in which they are placed. The first of these inquiries has hitherto been altogether neglected, from the impossibility experienced of attaining philosophical knowledge upon the subject; and while the first is unknown, any opinion formed upon the second must necessarily be imperfect. Phrenology affords the means of overcoming the difficulties of attaining information respecting the natural dispositions. The natural energy of the propensities, sentiments, and intellectual faculties, is in proportion to the size and activity of the organs, and these can be ascertained by observation.

"The Society, therefore, with the view of ascertaining the special combination of mental faculties, which exposes individuals most particularly to the temptation of committing crimes, have endeavoured to procure casts of the heads and skulls of as many criminals as possible. Reports on the conduct and development of four individuals of this description, illustrated by casts of their heads, were read to the Society.

"The

" The following reports and notices have also been read :

" Report on the Skull of King Robert Bruce, with Observations on the Character which it indicates, compared with his History; by Mr. James Law.

" Observations on the Talents of several distinguished Individuals, as indicated by the Development of their Heads, shown in their Portraits produced to the Society; by Mr. James Stewart.

" Historical Notice of early Opinions regarding the Functions of the Brain; by Mr. W. C. Trevelyan and Mr. George Combe.

" Notice of Cardan, the Philosopher, and of some Peculiarities of his Character, indicating a particular Endowment of several Faculties; by Mr. W. C. Trevelyan.

" No object is more interesting to Society, than the allotment of particular professions or occupations to individuals most fitted by nature to pursue them with advantage. Phrenology, by means of the cerebral development, affords a powerful help for discovering the natural dispositions and talents of individuals. The chief difficulty which remains, is to predict the effects of particular combinations of the primitive powers, and of particular modes of education upon them. Experience only can lead to certain knowledge upon these points; but experience can be obtained only by experiment and observation. The Society, therefore, by the kindness of a lady, who takes an interest in the science of Phrenology, has been made acquainted with the case of a girl of nine years of age, selected for education to a particular pursuit, on account of her cerebral development appearing eminently to fit her for such an avocation. A cast of the head has been made, and a report of her development and endowments at the time when her instruction commenced, has been placed among the records of the Society; and time will show how far the anticipations formed have been well or ill founded. It is three months since the course of instruction was begun; and hitherto the indications have surpassed, rather than fallen short of, the expectations entertained."

A list of the Members composing the Society is attached to the Report: ordinary Members forty-two; honorary Members three; corresponding Members five.

CEYLON LITERARY SOCIETY.

A Society for investigating the Natural and Civil History, Geography, &c. of Ceylon, was established under the patronage of the Hon. the Lieutenant Governor, at a meeting of Gentlemen of His Majesty's service, civil and military, held at the King's House in Colombo, the 11th of last December. The objects to which the attention of the Society seem principally to be directed are:

1st. " The Geography, Geology, and Mineralogy of Ceylon.

2dly.

2dly. "Its Botany, perhaps the richest and least exhausted of any in the world. In this branch the history of the Cinnamon tree, the various Palms so important to the sustenance of the people, the Rice, and the numerous other kinds of grains cultivated in the island; and modes of improving agriculture, well deserve very particular investigation.

3dly. "The Fishes of Ceylon, so various and yet almost undescribed; its Conchology, in which the Trincomalee and Manar districts particularly are so abundant, its Quadrupeds, Birds, Insects, and Amphibia including Serpents, afford subjects highly important for consideration.

4thly. "For the study of the Civil History, Language and Customs of the People, the facility of communication with the Kandyans offers advantages not hitherto enjoyed; and as the active curiosity of the Members will probably furnish to the Society much to illustrate the antiquities and topography of the country, as well as the other points to which its labours will be directed; the establishment of a Museum, which is proposed as part of the system, will serve to bring together specimens applicable to all these various heads, contributions to which are earnestly solicited from the public at large.

"The Fund to be raised by the Subscription of the Members, will be applicable to the hire of a house for the meetings of the Society, and for its Museum (unless it should please Government in patronage of the plan to assign it a building gratuitously for these purposes)."

The Society at its first meeting could boast of no less than fifty-one Members, all emulous for the success of the Institution. The Hon. Major-General Sir E. Barnes, the patron, was elected President. The Hon. Sir Hardinge Gifford; the Hon. Sir Richard Ottley; the Hon. R. Boyd, Esq.; the Hon. J. W. Carrington, Esq.; the Hon. and Venerable Dr. Twisleton and Dr. Farrell were elected Vice-Presidents; and the following gentlemen were named as a General Committee for managing the concerns of the Society, till the first meeting in 1822 (the Committee thenceforward to be elected annually), viz.

Lieut.-Col. Wright; Lieut.-Col. Walker; Dr. Dwyer; W. Granville, Esq.; A. Moon, Esq.; G. Turnour, Esq.; J. Deane, Esq.; Major Delatre; J. G. Forbes, Esq.; Rev. C. Lyon; H. A. Marshall, Esq.; Lieut. Gascoigne; Rev. J. G. Glenie; Lieut.-Col. Hamilton; Lieut. Thompson.

This General Committee divides itself into three Sub-committees of five Members each; viz. 1st, of Natural History and Agriculture; 2dly, of Geology, Mineralogy, and Geography; 3dly, of Civil History, Languages, and Antiquities.

Natives of respectability are eligible as honorary Members.

LXXXVI. *In-*

LXXXVII. *Intelligence and Miscellaneous Articles.*

Chatham, June 2, 1821.

Sir,—If any of the readers of the Philosophical Magazine will favour me with the following information, I shall feel much obliged.

Suppose a floating body of a cubical form, one-third of which is above water, Where is the point situated, by which it can be moved in a horizontal direction with the least effort? If this subject has been noticed by any writer, under what title is it to be found? By inserting this, and the answer, if it is favoured with one, in your very valuable miscellany, you will much oblige

Your most obedient servant, &c.

To the Editor of the Phil. Mag. J. K. K.

LARGE REFLECTING TELESCOPE.

Mr. J. Ramage, of Aberdeen, has constructed a 25 feet reflecting telescope, the speculum of which is 25 feet focal length, and 15 inches diameter, bearing magnifying powers from 50 to 1500. This is the largest telescope of the kind ever made, except Sir W. Herschel's. The mechanism by which the observer and the instrument are moved, is simple and well contrived.

TRAVELLERS.

An English traveller of the name of Cochrane has reached Irkutsk on foot, on his road to America, by the north-east promontory of Asia. On the 13th of September last, he had travelled 8000 versts in 123 days entirely on foot. He sleeps in the open air, and wears nankeen breeches.

Mr. Campbell, the Missionary, has returned from a second journey in South Africa. On this occasion he penetrated 800 miles from Cape Town, a greater distance than any other traveller had before penetrated, and considerably beyond Latakoo. He has discovered several large towns; some containing 10,000 or 12,000 inhabitants. The people were found friendly and docile, possessing some skill in the manufacture of pottery, in smelting of iron, and other arts, and so intelligent as to know the value of and wish for the introduction of better informed artizans. They likewise desire to have missionaries sent among them.

Messrs. Waddington and Hanbury, two of our adventurous countrymen who have visited Upper Egypt, may be expected in England by the end of this year. They are said to have discovered the city of ancient Meroe, spoken of by Herodotus, lib. ii. cap. 29; by Diodorus Siculus, i. 33; by Strabo, xvii.; and by Josephus, ii. 10. It was anciently called Saba, which name was changed to that of Meroe by Cambyses, in honour of his wife or sister.

NATIVE OXIDE OF CHROME *.

The combinations of this metal with two others, namely lead and iron, under different forms, have for some time found a place in our catalogues of minerals. A place must now also be made for chrome itself in that division of mineralogical systems which is allotted to the metals. I am not aware at least, that the oxide of chrome has yet been found by any one in a native state; certainly it has not been enumerated in any system of mineralogy.

I have recently discovered it here in Shetland, in the island of Unst. It is found in cavities in the chromate of iron, which abounds in this island, so as, for the space of many miles, to be scattered over the surface of the ground, and even to be used in common with the loose stones which it accompanies in the building of dykes.

This oxide is easily recognised by its beautiful green colour, and does not seem to differ from the green oxide produced in our laboratories by the action of heat. In some places it is merely diffused through the fissures of the ore; in others it occupies cavities resembling those of the amygdaloids. It is sometimes found in a powdery form; but at others it is compacted into a solid substance, bearing the marks of a crystalline structure, and somewhat translucent. Although it appears to be in abundance, when the specimens that contain it are broken, that effect is only the consequence of the brilliancy and contrast of its colour with the black and dark gray of the surrounding chromate of iron. It would be very difficult to collect many grains of it in a separate state from any of the fragments of the black ore which I examined.

The green oxide is accompanied by a yellow oxide of chrome, in cavities generally distinct from it, but sometimes intermixed, and in somewhat less abundance. This latter is more generally in the form of powder than the green. As the green oxide of chrome changes to yellow by heating it, M. Vauquelin appears to think that these are distinct oxides; but this point does not seem to have as yet been very satisfactorily examined. For the present purposes it will, at any rate, be more convenient to consider them merely as varieties of one mineral species. Those mineralogical writers who are desirous of increasing the number of species may easily follow a different course.

The mineral distinction of the oxide of chrome may be comprised in the following terms:

Oxide of Chrome.—This mineral is of a bright grass green colour, or else pale yellow; and is found either in a powdery or a compact form. In the former case, the aspect is dull; in the

* Journal of Science, No. 21.

latter,

latter, the lustre resembles that of compactly crystallized limestone, or marble. It either invests surfaces, or fills cavities in chromate of iron.

Its specific gravity has not been examined. It is soluble boiling in the alkalies, and communicates to them a green colour, but the solution is decomposed by further boiling, and the oxide is precipitated. By this character, and by its communicating a green tinge to glass, before the blow-pipe, it may be recognised and distinguished. It occurs in Unst, one of the Shetland isles.

Lest your readers should conceive that I had fallen into an error, in describing this mineral as new, I ought to add to this communication, that the oxide of chrome, described in Monsieur Lucas's arrangement of minerals, is a very different substance, and, I may add, improperly named. I need not quote from a book which is in the hands of many mineralogists. It is sufficient to remark, that his mineral is a compound substance, into which the oxide in question enters only as an ingredient. It would be proper that its name should be changed, to prevent confusion; the right of possession is clearly in the present substance.

I am yours, &c.

Shetland, August 1820. J. MacCulloch.

CURIOUS GEOLOGICAL FACTS.

The following curious fact was stated in the Quarterly Review, No. 43, p. 52, in an account of the quarries of marble whence the blocks are taken for the construction of the Plymouth break-water:

"The quarries are situated at Oreston, on the eastern shore of Catwater; they lie under a surface of about twenty-five acres, and were purchased from the Duke of Bedford for 10,000*l*. They consist of one vast mass of compact close-grained marble, many specimens of which are beautifully variegated; seams of clay however are interposed through the rock, in which there are also large cavities, some empty, and others partially filled with clay. In one of these caverns in the solid rock, fifteen feet wide, forty-five feet long, and twelve feet deep, filled nearly with compact clay, were found imbedded fossil bones belonging to the rhinoceros, being portions of the skeletons of three different animals, all of them in the most perfect state of preservation, every part of their surface entire to a degree which Sir Everard Home says he had never observed in specimens of this kind before. The part of the cavity in which these bones were found was seventy feet below the surface of the solid rock, sixty feet horizontally from the edge of the cliff where Mr. Whitby began to work the quarry, and one hundred and sixty feet from the original edge by the side of the Catwater. Every side of the cave was solid rock: the inside had no incrustation of stalactite, nor was there any ex-

ternal communication through the rock in which it was imbedded, nor any appearance of an opening from above, being inclosed by infiltration. When, therefore, and in what manner these bones came into that situation, is among the secret and wonderful operations of nature, which will probably never be revealed to mankind."

Professor Silliman having given a place to the foregoing in his American Journal of Science, No. 5, subjoins to it the following extract, translated from Count Bournon's Mineralogy, as a fact still more interesting:

"During the years 1786, 7, and 8, they were occupied near Aix in Provence, in France, in quarrying stone for the rebuilding, upon a vast scale, of the Palace of Justice. The stone was a limestone of a deep grey, and of that kind which are tender when they come out of the quarry, but harden by exposure to the air. The strata were separated from one another by a bed of sand mixed with clay, more or less calcareous. The first which were wrought presented no appearance of any foreign bodies; but, after the workmen had removed the first ten beds, they were astonished, when taking away the eleventh, to find its inferior surface, at the depth of forty or fifty feet, covered with shells. The stone of this bed having been removed, as they were taking away a stratum of argillaceous sand, which separated the eleventh bed from the twelfth, they found stumps of columns and fragments of stones half wrought, and the stone was exactly similar to that of the quarry: they found moreover coins, handles of hammers, and other tools or fragments of tools in wood. But that which principally commanded their attention, was a board about one inch thick and seven or eight feet long; it was broken into many pieces, of which none were missing, and it was possible to join them again one to another, and to restore to the board or plate its original form, which was that of the boards of the same kind used by the masons and quarry men: it was worn in the same manner, rounded and waving upon the edges.

"The stones which were completely or partly wrought, had not at all changed in their nature, but the fragments of the board, and the instruments, and the pieces of instruments of wood, had been changed into agates, which were very fine and agreeably coloured. Here then (observes Count Bournon) we have the traces of a work executed by the hand of man, placed at the depth of fifty feet, and covered with eleven beds of compact limestone: every thing tended to prove that this work had been executed upon the spot where the traces existed. The presence of man had then preceded the formation of this stone, and that very considerably, since he was already arrived at such a degree of civilization that the arts were known to him, and that he wrought the stone and formed columns out of it."

AGRI-

AGRICULTURE.

Major-General Beatson, a practical and experimental agriculturist, has addressed his plan of culture to the farmers of the three kingdoms, and has made an estimate of his expenses in cropping 29 acres of wheat at Knowle Farm, near Tunbridge Wells. He states that his practice has proved, that the cheap and universal manure of clay-ashes on calcined or *roasted* soil, with the stubbles, &c. on the land, is sufficient and preferable for corn crops on stiff soils, and much other land, to lime or dung, and may be applied at the expense of 20*s.* an acre, instead of the Sussex and Hampshire practice of lime and marl at 7*l.*, and of dung or other manures of an equal or greater cost. He has also invented a new implement of much power, and various application to the soil, as a general substitute for the ploughs and harrows in common use. This instrument pulverises the soil, and prepares it for corn crops, with one horse instead of four, and will go over three acres a day at an expense of 10*s.* 1*d.* to 11*s.* 4*d.* per acre only. He recommends the disuse of naked summer fallows in almost all cases. The whole expense of *his* cultivation for wheat, in rent, taxes, seed, cattle, labour, and manure, for the *present* year, is only 5*l.* an acre; and his crop of wheat, allowing only 20 bushels an acre, instead of 30 to 40, as last harvest, will cost no more than 40*s.* a quarter. The cost of growing an acre of wheat in Sussex has been stated to be 16*l.* In Yorkshire 120 bushels of bones, at 2*s.* 6*d.* a bushel, *have been* applied to one acre of land, to force crops of 32 to 35 bushels. In 1815, Mr. James Buxton, in evidence before the House of Commons, showed, in three statements, that the *average* expense for an acre of wheat in Essex was 14*l.* 15*s.* 11*d.* Lord Nugent, in his letter to Mr. Baker, in December last, writes, "Farmers are suffering, not because the produce is too cheap, but because the means of raising it are too dear;" and adds— "the more cheaply the food of man can be supplied, *surely the better*, if it be sold at a rate which will afford a fair return to him who grows it." General Beatson, from all his experience of practice, is thoroughly convinced of the advantages of his method over the old Sussex plan, with which he particularly contrasts it; and he appeals to the facts and result of the expense of his cultivation, and the produce of his land, and its condition, at the next harvest, of which the observation and the proofs will be palpable. A crop of wheat of average produce, compared with similar soils, and grown at a cost of 40*s.* a quarter, differs widely from the common claim of 80*s.* for a remunerating price, and the limit of protection from foreign import; particularly when the very moderate produce of only 20 bushels an acre is calculated.

 The

The Indian and Chinese methods of well pulverising, without *turning* the soil, may, it is thought, be practised with success to a great extent in Britain. One ploughing for wheat, though he has not considered it *necessary*, has been adopted *this* year; and ridge-ploughing in the winter, for the spring crops, which keeps the land dry, and exposes it to the action of the air and frost. The wheat stubbles, with a few faggots, have been used in burning a considerable breadth of soil. By using the wheat stubble as fuel, 30 to 35 loads of *soil and stubble ashes* have been made per acre *on the land*. The same stubble, had it been collected and carried at a great expense from the land, would not have yielded more than 10 loads of dung from the dung-heap. Besides the œconomy of making manure in the field on which it is to be laid, the operation of raking out the roots tends to clear the land much; and it may in this manner, with the new implement, be made, in a very short time, as clean as a garden. None can contend that *œconomy* in the processes of cultivation is not the *best* means for completely relieving the agriculturist from the weight of that load that now oppresses him.

CANAL BETWEEN THE ATLANTIC AND PACIFIC.

[From the National Intelligencer.]

Among many advantages of a commercial nature which would infallibly spring from the emancipation and attendant independence of South America, the greatest perhaps of all has hitherto been little noticed. The most momentous event in favour of the peaceful intercourse of nations, which the physical circumstances of the globe present to the enterprise of man, is the formation of a navigable passage across the isthmus of Panama. It is remarkable, that this magnificent undertaking, pregnant with consequences so important to mankind, and about which so little is known in this country, is so far from being a romantic and chimerical project, that it is not only practicable but easy. The river Chagre, which falls into the Atlantic at the town of the same name, about 18 leagues to the westward of Porto Bello, is navigable as far as Cruzes, within five leagues of Panama.

But though the formation of a canal from this place to Panama, facilitated by the valley through which the present road passes, appears to present no very formidable obstacles, there is still a better expedient. At the distance of five leagues from the mouth of the Chagre, it receives the river Trinidad, which is navigable to Embarcadoro, and from thence to Panama is a distance of 30 miles, through a level country, with a fine river to supply water for the canal, and no difficulty whatever to counteract the undertaking.—The ground has been surveyed; and not the practicability only, but the facility of the work completely ascertained. The

The important requisite of safe harbours, at either extremity of the canal, is also supplied to the utmost extent of our wishes. At the mouth of the Chagre is a fine bay, which received the British 74-gun ships in the year 1740, when Captain Knowles bombarded the Castle of St. Lorenzo; and at the other extremity is the famous bay of Panama. Nor is this the only expedient for opening the important navigation between the Pacific and Atlantic Oceans. Further north, is the lake of Nicaragua, which by itself almost extends the navigation from sea to sea. Into the Atlantic Ocean it falls by a navigable river, and reaches to within three leagues of the Gulf of Paparayo in the Pacific.

Can we refuse to dwell for a moment upon the prospects which the accomplishment of this splendid but not difficult enterprise opens to the United States, as well as to Europe? It is not merely the immense commerce of the western shores of South America, extending almost from pole to pole, that is brought, as it were, to our very doors; but immense would be the traffic which would immediately begin to cover that ocean denominated Pacific. All the riches of India and China would move towards America. The riches of Europe and America would move towards Asia. Vast depôts would be formed at the great commercial towns, which would immediately arise at the two extremities of the central canal.

And is it too much to hope that China and Japan themselves, thus brought so much nearer the influence of American and European civilization, much more constantly and powerfully subject to its operation, would not be able to resist the salutary impressions, but would soon receive important changes in manners, arts, ideas, and institutions? The hope rests on such strong foundations, that it seems to rise, upon contemplation, even to a certainty! And what results might not be expected for the whole of Asia, that vast proportion of the earth, which in its most favoured parts has been, during all the latter ages, condemned to demi-barbarism and the miseries of despotic power! It may, however, be considered as certain, that South America, which stands so much in need of industrious inhabitants, would receive hosts of laborious Chinese, who already are to be found in all parts of the Eastern Archipelago in quest of employment and of food. These are a few of the results which there is reason to expect from a regulation of the affairs of South America. '*Tempora mutantur, et nos mutamur in illis.*' F. M.

FOSSIL CROCODILE.

The following is an extract of a letter from M. Cuvier to the Royal Academy of Sciences at Caen, returning thanks for a well executed model which it had sent that learned anatomist of a fossil

fossil crocodile lately discovered in the neighbourhood of that city.

"It is now certain that this crocodile is of a species quite peculiar, and different not only from all living crocodiles, but from all fossil crocodiles hitherto discovered. The only one which comes near it, is that dug up near Pappenheim, and which is preserved in the Cabinet of the Royal Academy of Bavaria."

A LIZARD FOUND IN A MILL-STONE.

A short time since, as David Virtue, mason, at Auchtertool, a village four miles from Kirkaldy, in Scotland, was dressing a barley mill-stone from a large block, after cutting away a part, he found a lizard imbedded in the stone. It was about an inch and a quarter long, of a brownish yellow colour, and had a round head, with bright sparkling projecting eyes. It was apparently dead, but after being about five minutes exposed to the air it showed signs of life. One of the workmen, very cruelly, put snuff in its eyes, which seemed to cause it much pain. It soon after ran about with much celerity; and after half an hour was brushed off the stone and killed. When found, it was coiled up in a round cavity of its own form, being an exact impression of the animal. This stone is naturally a little damp; and about half an inch all round the lizard was a soft sand, the same colour as the animal. There were about 14 feet of earth above the rock, and the block in which the lizard was found was 7 or 8 feet deep in the rock; so that the whole depth of the animal from the surface was 21 or 22 feet. The stone had no fissure, was quite hard, and one of the best to be got from the quarry of Cullaloe—reckoned perhaps the best in Scotland.

LUSUS NATURÆ.

A person of the name of Robinson, has obtained and brought to New York, from the Indian country near Mackinac, an Indian, having in each arm and leg more than double the number of joints ordinarily allowed to man by Nature. This extraordinary being is in a measure helpless, and unable to stand, yet he has discovered a contrivance by which he obtains locomotion—this is a large wooden bowl, in which he rolls himself along with considerable facility when on a smooth and level surface. This Indian is said to be quite intelligent, speaking the tongues of three or four different tribes, and conversing fluently in the common French of the country.

Mr. Robinson mentions that he saw, while in the Indian country, what he deems a far greater curiosity. This is an Indian, whose body is thickly covered with long hair. The hair on the outside of his hands and fingers, which is permitted to grow, is stated

stated to be so long that he is enabled to tie it round his wrists. His forehead, nose, and every part of his face is said to be covered with hair. The Indians of his tribe pay him much respect in consequence of his superior sagacity and hardiness.

THE BOA CONSTRICTOR.

On the 6th of March last, there was killed at Sandy Bay, Jamaica, a large serpent of the species of the *Boa* of Cuvier. It is thus described by a writer in the Kingston Gazette:—The jawbone, the palate bones, and the other bones of the mouth, are attached to each other and to the cranium by elastic ligaments, which, by stretching, allow the dilatable throat to receive bodies of dimensions larger than the mouth in its ordinary or quiescent state. Each upper and lower jaw-bone, and each palate-bone, is furnished with a row of sharp, fixed, unpierced teeth, curved backwards, so that the mouth contains six nearly parallel rows of teeth, four above and two below. The windpipe is very long, and there is but one lung. The tail is prehensile, and has at its root two horny hooks or claws, something like the spurs of a cock. Along the back there runs a broad chain, formed of large, irregular, hexagonal, blackish spots, alternately with others which are pale, and of an oval shape. Scales under the body and tail, single and transversal. Such is the Boa, as described by Cuvier, and such exactly is the description of the animal found at Sandy Bay. It was fourteen feet long, and its greatest diameter when jejune was seven inches: when killed, it was gorged apparently with a kid or a lamb.

This species of snake is very common in the southern continent of America, where it sometimes grows to the length of 30 or 40 feet, and is a formidable foe to sheep, deer, goats, and (according to some accounts) even to cattle.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Thomas, of Sithney, Cornwall, merchant; and Joseph Lobb, of Sithney, farmer, for a machine or instrument for cutting and preparing lay or sea ground for tillage at much less expense and in a shorter space of time than are required by the present mode of ploughing; and also for renewing grass land, lay or sea ground, with seeds, without destroying or tearing up the whole of the surface thereof.—Dated 1st May 1821.—2 months allowed to enrol specification.

To Alexander Law, of the Commercial Road, Mile End, founder, for improvements in the formation of bolts and nails for ship and other fastenings.—1st May.—2 months.

To Robert Delap, of Belfast, merchant, for certain improvements in producing rotatory motion.—1st May.—6 months.

To Richard Jones Tomlinson, of Bristol, merchant, for his improved rafter for roofs or beams or for other purposes.—3d May. —6 months.

To John Reedhead, of Heworth, county of Durham, engineer and mariner; and William Parrey, of East-lane, Walworth, master mariner, for certain improvements in propelling vessels.—5th May.—6 months.

To Aaron Manby, of Horsley, near Tipton, ironmaster, for certain improvements in the making and manufacturing of steam-engines.—9th May.—2 months.

To George Frederick Eckstein, of High Holborn, ironmonger, for certain improvements in cooking apparatus.—9th May.—6 months.

To John Mayor, of Shawbury, county of Salop, and Robert Cook, of Shrewsbury, accountant, for certain improvements in the machinery for raising water, which they intend to denominate *Hydragogue.*—9th May.—6 months.

To Samuel Hall, of Basford, county of Nottingham, cotton spinner, for improvements in the manufacture of starch.–9th May. —6 months.

To Robert Paul, of Starton, county of Norfolk, gentleman, and Samuel Hart, of Reden Hall with Harleston, same county, painter and gig-maker, for certain improvements in springs applicable to various descriptions of carriages.—17th May.—6 months.

To Sir William Congreve, of Cecil-street, Strand, baronet; and James Nisbet Colquhoun, of Woolwich, lieutenant in the Royal Artillery, for certain improvements in the art of killing and capturing whales, and other animals to which such means are applicable.—7th June.—6 months.

To John Vallance, of Brighton, brewer, for a method and apparatus for freeing rooms and buildings (whether public or private) from the distressing heat sometimes experienced in them, and of keeping them constantly cool, or of a pleasant temperature, whether they are crowded to excess, or empty; and also whether the weather be hot or cold; and that in some cases with and in some cases without a gas or gases, extended or additional applications of the principles, or of some or one of the principles (either of construction or operation) thereof, as applicable to purposes other than what he first contemplated.—19th June.—6 months.

BAROMETRIC OBSERVATIONS.

Crumpsall, Lancashire, June 12, 1821.

Sir,—I send you the observations made at this place on Monday the 11th instant; and those also, made on the same day, at Manchester, by Mr. Hanson.

CRUMP-

CRUMPSALL.

1821. A.M.	Bar.	Ther. att.	Ther. det.	Wind.		Weather.
June 11th 8h.	29·650	52°	51°	N.E	brisk.	Sunshine with clou.
9	29.660	51·5	51	N.E.	fresh.	Do.
10	29.680	52·7	53	N.E.	brisk.	Do.
11	29.685	53	54	N.E.	do.	Do.
12	29.700	54	56	N.E.	high.	Do.
1	29·716	54	55	N.E.	brisk.	Do.

MANCHESTER.

1821. A.M. June 11th	Bar.	Ther. att.	Ther. det.	Wind.		Weather.
8h.	29.880	55·5	54	N.E.	brisk.	Fine.
9	29.885	57·5	55	N.E.	fresh.	Cloudy.
10	29.900	57·5	57·5	N.E.	do.	Fine, but cloudy.
11	29.920	58·5	57·5	N.E.	brisk.	Cloudy, with sunsh.
12	29.925	61	57·5	N.E.	do.	Fine, with few clou.
1	29·940	63	58	N.E.	do.	Fine.

From the observations made on the 14th of May, that have already appeared, it seems that the barometer was not so stationary on that day in some parts of the country, as it was here, and at Manchester. According to the observations of Mr. Cary in page 400, it appears to have been steady from nine o'clock to one, at the place where they were taken; but at Bushy-Heath, and Northampton, its fluctuations were frequent, and considerable.

It has not yet been ascertained, I believe, over what extent of country it is usual for the variations of the barometer to be similar on the same day; nor in what manner these variations are influenced by the force and direction of the wind.

To determine these two important points with a tolerable degree of precision, would require a well arranged plan of observation, and the mutual co-operation of a number of individuals stationed in different parts of the kingdom: and when it is considered what curious and interesting subjects of inquiry these are, they will not be thought undeserving of the particular attention of meteorologists. I am, sir, your obedient servant,

To the Editor. JOHN BLACKWALL.

Arundel, June 13, 1821.

SIR,—I send you the Barometric Observations made at this place, on the 14th of May, and 11th of June. The height of the basin of the Barometer above the level of the sea, at low water, I estimate to be about 68 feet. Yours obediently,

To the Editor. G. CONSTABLE.

Hour.

Hour.	Barom.	Thermo. att.	Thermo. det.	Wind.	Weather.
May 14th,	°	°	°		
8h	29·385	50	48	N.W.	Cloudy. — Fresh breezes.
9	29·390	50	49	N.W.	
10	29·390	52	50	N.W.	
11	29·392	53	52	W.N.W	Showery.
12	29·400	53	52	N.W.	Cloudy.
June 11th, 8	29·915	52	51	N. by E.	
9	29·918	53	52	N. by E.	Clouds and sunshine. — Moderate breeezes.
10	29·933	53	53	N. by E.	
11	29·938	54	53	N.N.E.	
12	29·940	55	54	N.N.E.	Showery.—Fresh breeze.

Pocklington, Yorkshire, June 20, 1821.

SIR,—At the desire of my friend, Mr. T. Squire, of Epping, (who communicated to you my observations before) I send you some Barometrical remarks, &c. taken here on the 14th of May and the 11th of June. Those of the former day were done by a person in my absence: those of the latter I made myself with the greatest care I possibly could.

Hour.	Barom.	Thermom. att.	Thermom. det.	Wind.	Weather.
May 14th, 8h	29·15	51	48	S.W.	Clear and cloudy.
morning, 9	29·16	52	50		Cloudy.
10	29·16	53	50		Ditto.
11	29·16	54	52		Ditto.
12	29·16	55	46		Ditto.

Heavy showers this day.—Rain at half past 12 at noon; rain and hail at 2 o'clock; and rain at 5 P.M.

I was at Appleby in Westmoreland, this day; and in that place there were frequent showers of hail and rain—the mountains round about were capped with snow.

Mean time.	Barom.	Thermom. att.	Thermom. det.	Wind.	Weather.
June 11th, A.M. 7h	29·893	55·7	50·0	North.	Cool—rather windy:—clear and cloudy.
8	29·897	57·4	53·7	N. by E.	Cool and windy—clear, except some gray broken clo.
9	2[illegible]3	57·3	54·2	N.	Cool and windy—clear and cloudy: passing showers.
10	29·927	57·7	57·8	N. by E.	Cool & windy—clear, except some broken white clouds.
11	29·948	58·0	56·0	N. by E.	Cool & windy—dense clouds to the North—intervals of sunshine.
12	29·951	58·6	57·8	N. by W.	Rather windy—clear & cloud.

My

My Barometer is a very good one of the kind—it was the favourite one of the late Mr. H. Andrews, of Royston, which he used for his Weather Journal.

The observations of your correspondents, made on the second Monday in the month, will be of great use in finding out the height of different places from the level of the sea; and will probably lead to a more accurate knowledge of the causes of the amazing changes that take place in the atmosphere, with regard to its weight, temperature, &c.

Those who keep meteorological journals cannot but have remarked the mildness of January; the dryness and amazing barometrical pressure during the month of February*; the heat that prevailed towards the end of April, with much lightning and thunder; and then the cold weather which immediately followed, and continues to this day.

I am, sir, your obedient servant,
WILLIAM ROGERSON jun.

Leighton, June 22, 1821.

SIR,—The usual Barometrical observations made at Leighton, on the 11th instant, are as under:

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·685	49	47	N.	moder.	Little rain.
9	29·698	49	48	N.	calm.	Cloudy.
10	29·703	50½	51	N.N.E.	moder.	Rain.
11	29·723	49½	45	E.N.E.	do.	Rain.
12	29·742	50	46	N.N.E.	do.	Rain.
1	29·750	50½	47	N.E.	do.	Rain.

At Bushey, by Col. BEAUFOY.

1821.	Barom.	Ther. att.	Ther. det.	Wind.	Denom.	Weather.
8h	29·405	49	46	N.N.E.	fresh.	Cloudy.
9	29·413	49½	49	N.N.E.	do.	Do.
10	29·419	50	51	N.N.E.	do.	Do.
11	29·443	51	53	N.N.E.	do.	Do.
12	29·465	51½	52	N.N.E.	do.	Showery.

I had some expectation of being able to send you the result of Mr. Comfield's observations this month, but have not received them at present. In this place, I beg to correct your spelling of

* The mean height of the Barometer at Pocklington, in February, was 30·125! but in January it was only 29·45.

 Mr.

Mr. Comfield's name, which by mistake you have made Cornfield in last Number, p. 398.

Upon examining the construction of the best portable barometers, I have noticed one probable source of error in the position of the *attached Thermometer* which is fixed near the basin of mercury, and possibly may give nearly the heat of that portion of the mercury. But when it is considered that all fluids communicate heat very slowly *downwards*, and that much of the upper part of the tube is exposed to the atmosphere, and may become several degrees warmer than the quantity in the basin, or the wood in which the attached thermometer is fixed, it may cause the *average* heat of the mercury in the tube to be several degrees above that in the basin: but in the necessary calculations respecting the relative height of two places, it is the heat of the mercury *in the tube* that forms one part of the data, and not the temperature of that in the basin.

On the 11th instant, I suspended two thermometers near the centre of my tube, one on each side, and found a difference of 4 to 5° between the heat in that part, and the heat indicated by the attached thermometer: it is well known that a difference of 4 or 5° will frequently affect the result of a calculation ten or twelve feet, and may perhaps have caused some of those anomalies, which may in part be avoided by having the attached thermometer so fixed as to give the mean heat of the mercury *in the tube*, instead of that in the basin.

I am not so sanguine as to imagine the improvement above suggested will reconcile all the differences found; but it is a maxim with me to correct an error when I find it, and view it as one step permanently gained.

The customary correction for the heat of the atmosphere, as pointed out by the detached thermometer, I have long viewed with doubts.

I have calculated the relative heights of *Crumpsall* and *Leighton*, exclusive of fractions, to be nearly as follows:

Crumpsall above Leighton.

Feb.	..	174 feet.
March	..	161
April	..	159
May	..	123 through the medium of Bushy.

Col. Beaufoy has calculated the height of his instrument above that of Mr. Cary's in February 487·4 feet.

March	481·0
April	487·6
May	480·5
	484·1 mean.

Mr.

Mr. Cary's instrument being 73 feet above low water in the *Thames*, shows the height of Bushey to be 557 above the said low water. Yours truly,

B. BEVAN.

Meteorological Observations at Melville Island.

Abstract of the Register of the Thermometer and Barometer during ten months, at Winter Harbour, Melville Island, North Georgia, 1819 and 1820.

Latitude 74° 47′ 18″, Longitude 110° 48′ 30″ W.

Date.	Thermometer.				Barometer.			
	Maximum.	Minimum.	Mean.	Range.	Maximum.	Minimum.	Mean.	Range
	°	°	°	°	Inches.	Inches.	Inches.	Inches.
1819. October..	+17·5	−28	− 3·46	45·5	30·32	29·1	29·813	1·22
November	+ 6	−47	−20·6	53	30·32	29·63	29·945	0·69
December	+ 6	−43	−21·79	49	30·755	29·1	29·865	1·65
1820. January .	− 2	−47	−30·09	45	30·77	29·59	30·078	1·18
February	−17	−50	−32·19	33	30·15	29·32	29·769	9·83
March ..	+76	−40	−18·1	46	30·26	29·	29·803	1·26
April	+32	−32	− 8·37	64	30·86	29·4	29·978	1·46
May	+47	− 4	+16·66	51	30·48	29·25	30·109	1·23
June	+51	+28	+36·24	23	30·13	29·5	29·823	0·63
July ...	+60	+32	+42·4	28	30·01	29·13	29·608	0·88

REMARKS.—The thermometer was fixed, during the winter, on the south side of a david projecting from the ship's side, and was usually from 3° to 6° higher than one suspended freely in the air at a distance from the ship. This difference increased as the summer advanced, and the sun rose sufficiently above the horizon to heat the ship, amounting latterly to 15° or even 20° about noon. The thermometer was, of course, always shifted to the shaded side of the ship or david.

On the 15th of February, at 6 P.M., a thermometer suspended freely in the air at a distance from the ship stood at −55°, being the lowest degree registered during the winter.

The very low temperatures were invariably in calm and clear weather; the rise of the thermometer being the immediate consequence of a breeze springing up, and being proportioned to its strength.

The barometer rose with northerly and westerly, and fell with southerly and easterly winds; but it was not so decided that the indications preceded the changes, as it is stated to be in more southern climates.—*Journal of Science*, No. 21.

METEOROLOGICAL JOURNAL KEPT AT BOSTON, LINCOLNSHIRE,

BY MR. SAMUEL VEALL.

[The time of observation, unless otherwise stated, is at 1 P.M.]

1821.	Age of the Moon.	Thermometer.	Barometer.	State of the Weather and Modification of the Clouds.
	DAYS.			
May 15	14	54·	28·90	Fine
16	15	43·	29·58	Rain
17	full	61·	29·70	Fine
18	17	55·	29·75	Cloudy
19	18	59·	29·95	Fine
20	19	54·	30·	Ditto
21	20	53·	29·95	Ditto
22	21	50·	29·85	Cloudy
23	22	50·	29·70	Ditto
24	23	44·	29·80	Showery
25	24	55·	29·60	Cloudy—rain P.M
26	25	43·	29·65	Ditto
27	26	52·5	29·70	Fine—rain P.M.
28	27	46·	29·65	Ditto—rain A.M.
29	28	55·	29·85	Ditto
30	29	58·	30·03	Ditto
31	new	56·	29·95	Cloudy
June 1	1	65·	29·80	Fine
2	2	62·5	29·70	Ditto
3	3	63·	29·60	Ditto
4	4	61·5	29·37	Cloudy
5	5	72·	29·40	Fine
6	6	64·	29·50	Ditto
7	7	64·	29·34	Ditto—rain, with thunder and lightning P.M.
8	8	52·5	29·50	Cloudy
9	9	53·5	29·50	Fine
10	10	51·	29·50	Cloudy
11	11	55·	29·70	Fine—heavy rain A.M.
12	12	50·	30·05	Cloudy
13	13	53·5	30·03	Ditto
14	14	60·	30·13	Fine

METEORO-

INDEX TO VOL. LVII.

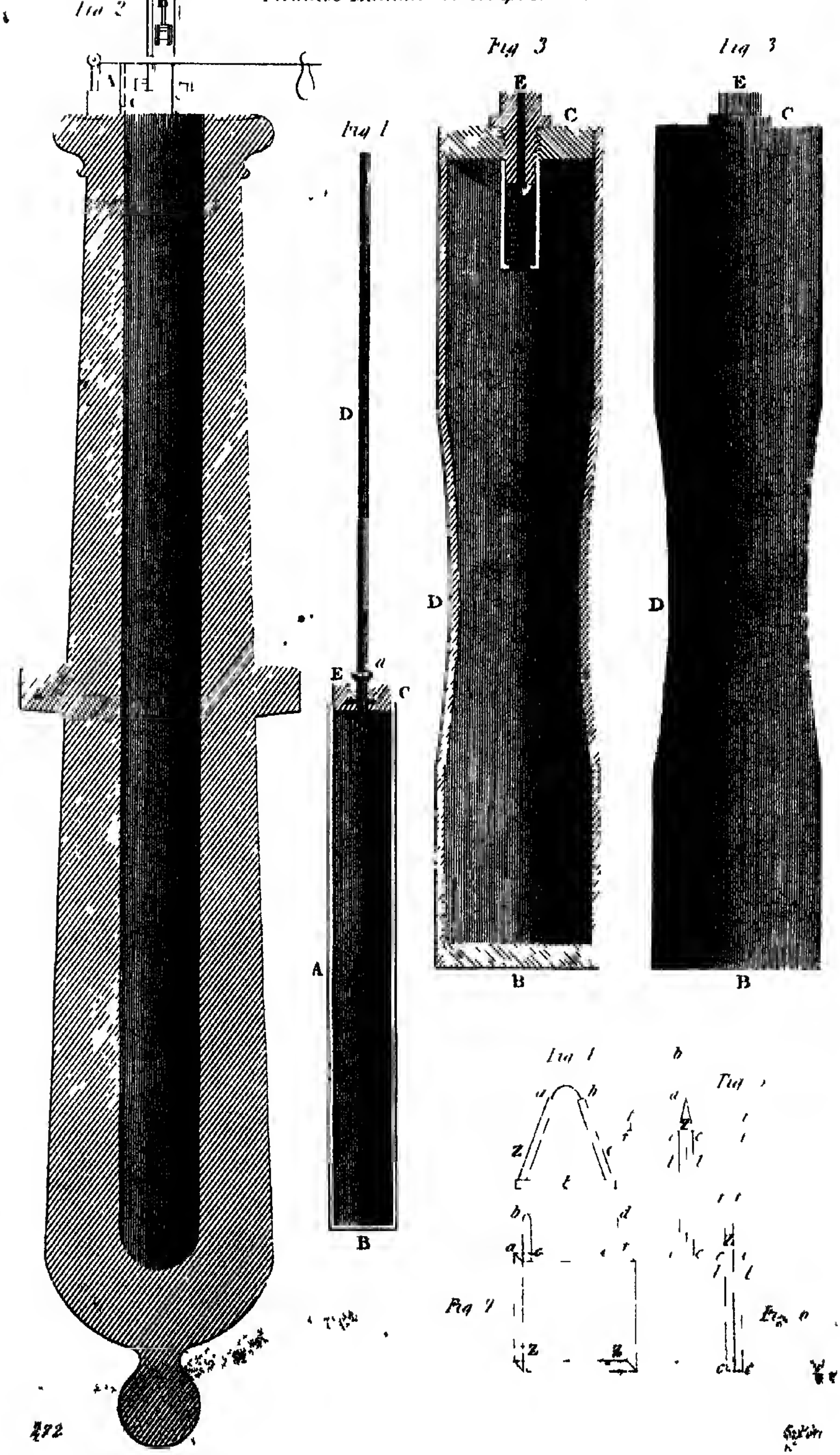
END OF THE FIFTY-SEVENTH VOLUME.

To the Binder.

The Binder is desired to cancel the Title-page published in the January Number; and to place the wood-cut illustrative of Dr. THORNTON'S Virgil before p. 361.

Printed by R. and A. Taylor, Shoe Lane.

Perkins's Machine for compressing Water



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Mr H C Jennings's Log Glass.

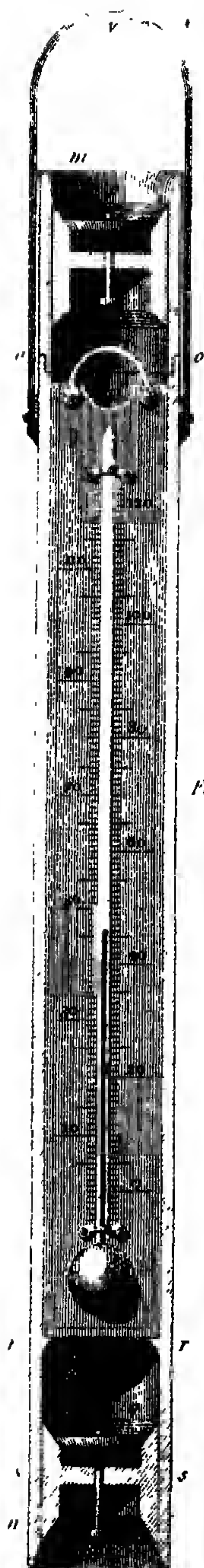


Fig. 3

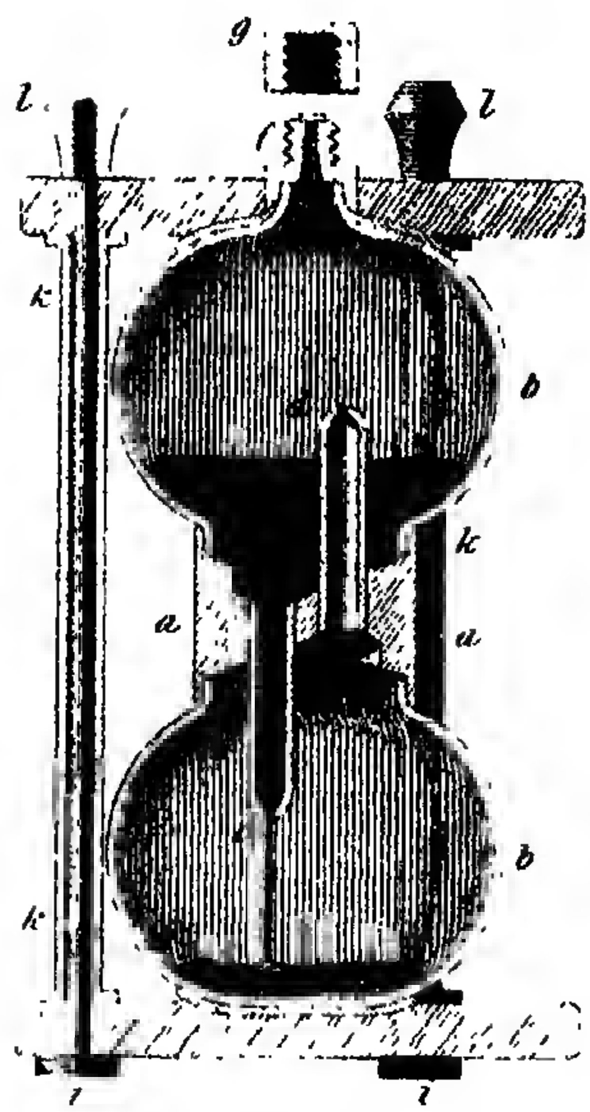


Fig. 1

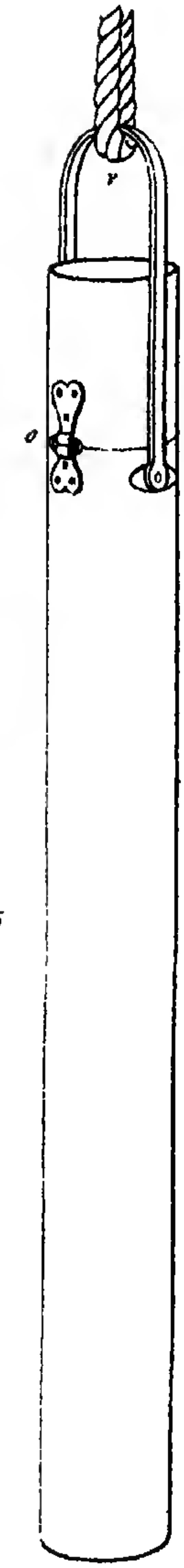


Fig. 5

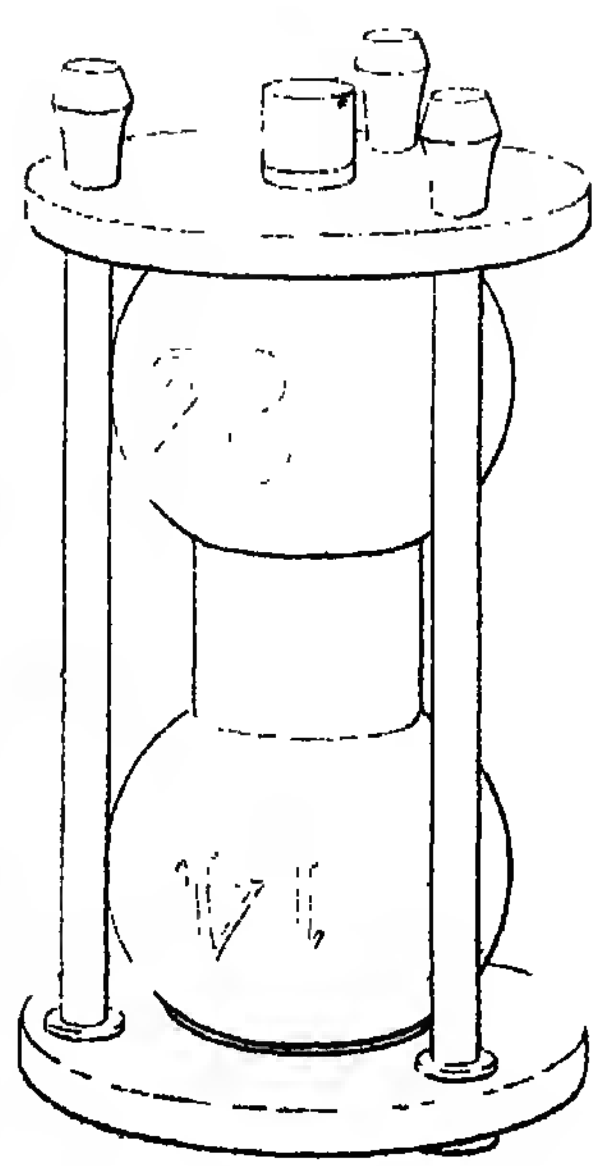


Fig. 2

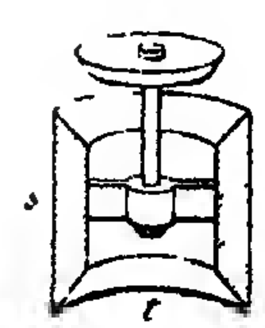
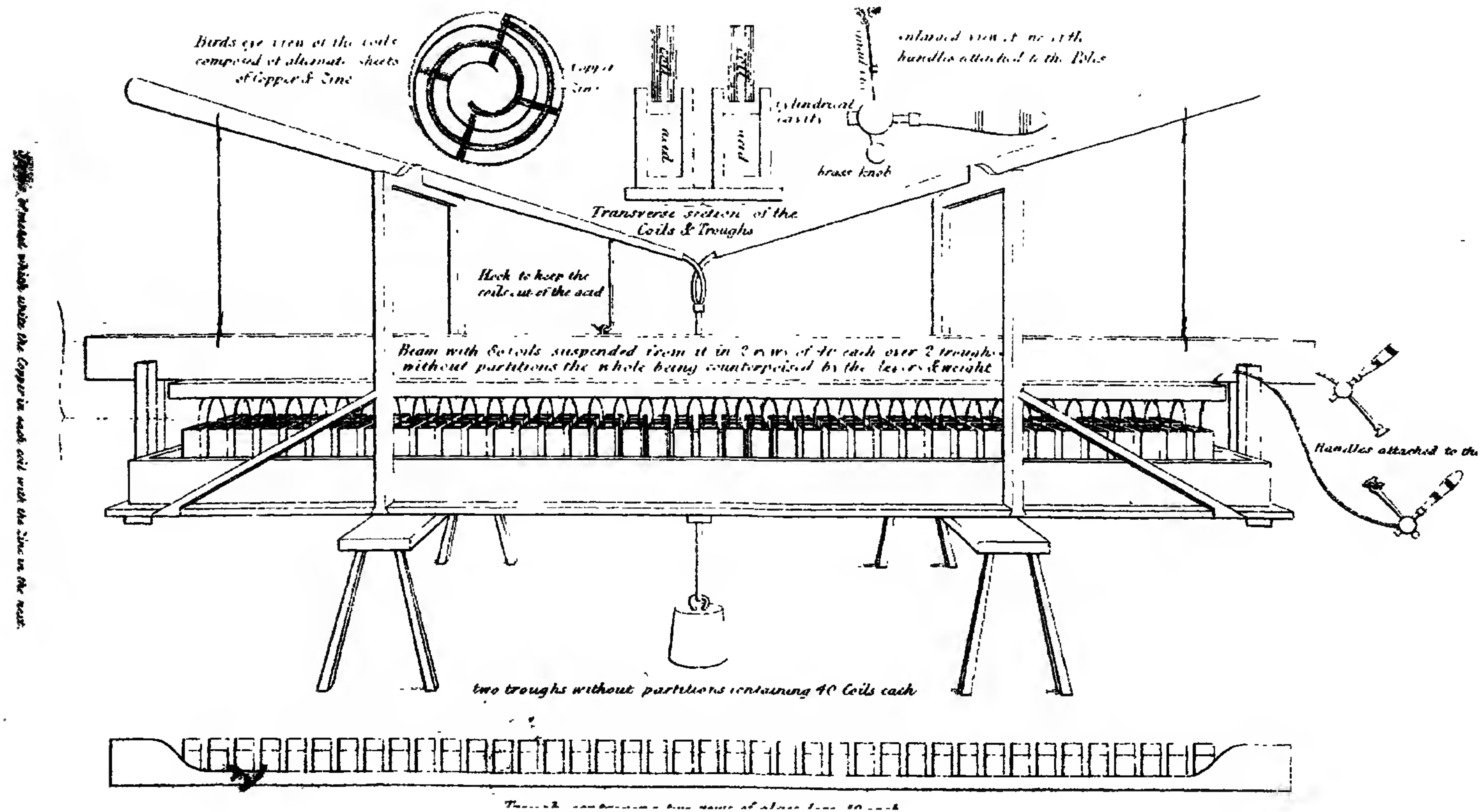
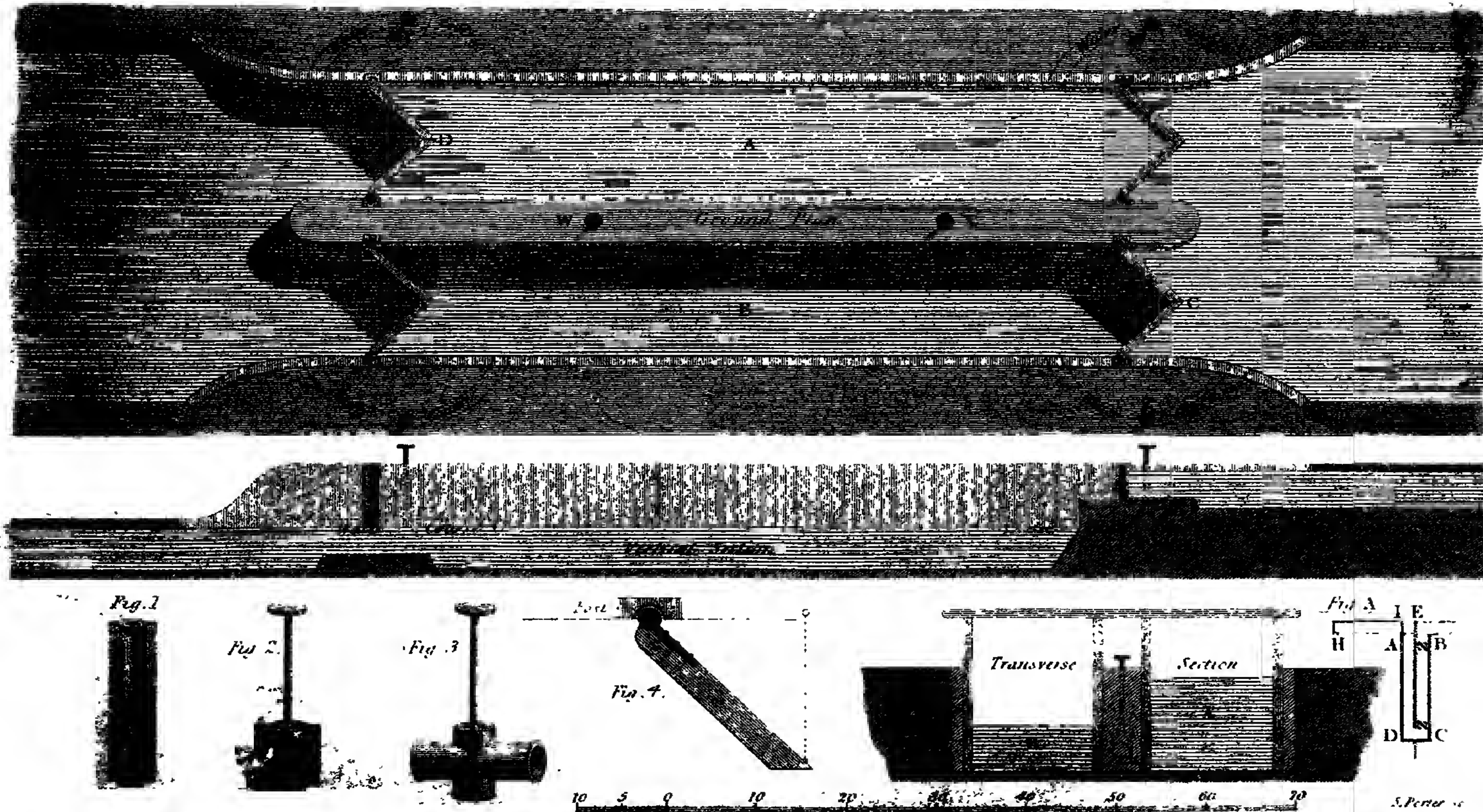


Fig. 4

Mr R Jameson's Marine Thermometer Case


Birds eye view of the coils composed of alternate sheets of Copper & Zinc
Copper
Zinc
coil
acid
Transverse section of the Coils & Troughs
cylindrical cavity
brass knob
enlarged view of one of the handles attached to the Poles
Hook to keep the coils out of the acid
Beam with 80 coils suspended from it in 2 rows of 40 each over 2 troughs without partitions the whole being counterpoised by the levers & weight
Handles attached to the
two troughs without partitions containing 40 Coils each
Strips of metal which unite the Copper in each coil with the Zinc in the next.


Ground Plan
Vertical Section
Fig. 1
Fig. 2
Fig. 3
Fig. 4.
Transverse Section
Fig. A
S. Porter sc